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VOL. XXI. No. 3 May, 1914

THE PSYCHOLOGICAL REVIEW

PRINCIPLES OF SELECTION IN ANIMAL LEARNING

BY HARVEY CARR

The University of Chicago

In comparative psychology, pleasure and pain are generally regarded as the sole selective principles in learning; the successful act is 'stamped in' by the pleasure, and the errors are eliminated by the unpleasantness. In opposition to this prevalent view, this paper will advance certain objective principles of explanation. Before stating these factors, we shall develop several distinctive features of the problem situation which are important from the standpoint of this paper.

The sensory situation which confronts an organism is a highly complex one. In a problem box, the stimulus consists not only of the food, but the problem box, the larger containing box, the table top upon which these are placed, oftentimes the experimenter and various objects in the room, and the motivating hunger stimulus. It is irrelevant whether hunger be due to the presence of a material stimulus or to the absence of a stimulus customarily present. Either sensory condition constitutes a stimulus from the psychological standpoint. Hunger is a persistent irritating sensory condition which arouses activity until some of the acts succeed in altering or alleviating the disturbing condition.

The animal does not react to this complex situation as a unitary whole, as a single stimulus. He reacts to it selectively, and as a series of stimuli. There is a circular interaction between the sensory stimuli and the animal's movements. Each act modifies the stimulus in some respect, and the change of stimulus in turn modifies the act. The motor excess, or the

variability of response to a problem, can be explained to a large extent in this manner. Every movement generates distinctive kinesthetic and cutaneous stimuli. The intensity and direction of the food odor varies with practically every movement. The visual situation differs for every attitude and position of the animal. Cutaneous elements may be added or subtracted from time to time. The successful act introduces gustatory elements and modifies the hunger stimulus. The motor effects of these sensory changes differ in degree and kind; they range from the extreme of strengthening and accelerating the act to its complete suppression due to the initiation of an antagonistic act. What these motor effects shall be depends upon the nature of the resultant stimulus and the animal's organization in reference to it. An act that brings an animal in close proximity to the food is accelerated not because of pleasure but because the animal is built that way. A resultant electric shock will suppress an act through the initiation of an avoiding response. The negative response is to be explained in terms not of unpleasantness but of the animal's innate organization to such a stimulus.

The stimulus to which the animal responds also changes radically from trial to trial throughout the learning process. The animal gradually ignores some aspects of the sensory situation and begins to respond to other aspects which at first were not noticed. In a problem of visual discrimination, a rat reacts at first to the food odor, the exits, the sides of the box —in fact to most anything and everything except the visual objects to be discriminated. He generally makes considerable progress in mastering the problem in terms of position and alternation before his behavior is affected by the crucial stimuli. While the series of responses to a problem are an expression of the animal's innate and acquired organization in reference to that sensory situation, yet these acts are *rarely* aroused by the crucial stimuli themselves. Hence most problems at least involve to some degree the formation of a new association between the crucial stimuli and acts which were originally made in response to other aspects of the sensory situation. It is not our purpose to discuss the method and conditions of forming

new connections. We merely note the fact that they do tend to become established. We have given, then, a system of connections between acts and the various sensory aspects of the problem; some of these connections are new while some represent the previous organization of the animal. The further progress of the learning consists of the preservation of some one, or group of these connections and the elimination of the others.

Selection and elimination are the diverse effects of a single process or mechanism. All connections tend to be preserved; all develop in strength and functional efficiency during the learning process, but their development proceeds unequally. The unsuccessful tendencies are not eliminated in the sense of being torn out by the roots; they are eliminated only in the sense of not being aroused in that situation. The strongest and most prepotent tendencies of the group function first and dominate the situation. The successful act is selected because it finally becomes the most prepotent in the group; all others are eliminated, or better are 'suppressed,' because of their lesser development in functional efficiency.

The problem of determining the various principles of selection and elimination thus resolves itself into a search for those factors which favor the retentive development of the successful act at the expense of the many failures. The principles are relative recency, relative frequency, and relative intensity. We shall develop our conception by applying these principles to the various types of problem. For our purposes, all animal problems may be divided into three classes: (1) that involving a series of simple acts directed toward the stimulus, the final one of which is necessarily the successful one. This type is illustrated by a problem box opened by pulling upon a cord suspended in front of the door. (2) The second type is represented by the maze or a complex problem box involving a fixed series of lever manipulations. The successful act is a complex whole composed of elements which were originally separated from each other by many useless acts. The process of learning involves the elimination of the useless and the serial coördination of the various elements, only one of which can be the final one of the series. (3) The third type deals with the inhibition of some instinct or habit.

All three principles are effective in the first problem. The successful act tends to occur more frequently during the learning than any of the useless acts. The successful act must occur in every trial, while as a matter of fact any useless act on the average does not occur in more than half of the trials. It is possible, however, for some error to be repeated a number of times in one trial. Since the successful act can occur but once per trial, it is thus possible for some error to be repeated throughout the learning process more frequently than the successful act. Why should not this error be selected? Our answer is that this fixing of useless acts does as a matter of fact often occur in both animal and human learning, and the phenomenon supports rather than disproves our conception. As to the final elimination of such useless acts, we are forced to contend that they never would be eliminated on the basis of frequency alone. Their final elimination is due to the coöperation of the other factors.

The successful act is intensified and accelerated by its sensory consequences to a greater degree than is any other act. This act possesses a number of peculiar and distinctive sensory consequences. The string in common with most other parts of the apparatus offers some resistance to attack but its behavior is unique in that it suddenly gives way to the pull. The sudden opening of the door is a surprising and unusual visual and auditory result that not only attracts attention but which often excites a certain timidity. After entrance, the box is sensed from the inside—a rather striking effect to those animals equipped with some disposition either to avoid or to seek such enclosures. The intensity of the food stimulus is increased in a more pronounced manner than heretofore, while the new elements of taste and mouth contact are added. The final and most important sensory consequence distinctive of the successful act is the alteration of the hunger aspect of the sensory situation. That the successful act is characterized by distinctive and important sensory effects is evident from the fact that the only real criterion of a success as opposed to an error or failure depends in the last analysis upon the nature of the sensory effects of those acts. Not only does the successful

act result in novel and significant sensory changes, but there is evidence on the motor side of their stimulative efficiency. The final series of acts beginning with the downward pull on the string represent the maximum of tension, excitement, vigor, decisiveness and acceleration. This change in the character of the animal's behavior is very apparent and striking though rather difficult to measure in any quantitative terms.

The successful act is necessarily the final one of the series—the most recent one. The effectiveness of this factor can naturally be inferred from human experiments. Any explanation of its efficiency is more difficult, but this difficulty obtains for the human realm as well. Although we are concerned more with matters of fact than with explanation, we may mention three possibilities. The successful act stands in a closer temporal relation to the subsequent trial than does any of the failures, and we can assume that the retentive effect of any act is inversely proportional, other things being equal, to its age. This disparity of age, however, would seem to be negligible except in those problems where a number of trials are given in immediate succession. Recency may also be interpreted as temporal contiguity to the food stimulus. In this sense the factor would seem to be a special case of the intensity principle. Again, the successful act may be favored because it is the *final* one, rather than an act which is followed by further activity. It is known that any activity interferes with the gradual setting or fixing of the retentive effects of any immediately previous act. The retention of all useless acts is thus retarded by the necessity for further activity, while the final or successful act is relatively favored by the absence of such subsequent distractions.

The maze is taken for the second type of problem. The effective factors are frequency and intensity. During the first trial the segments of the true path will be traversed more frequently than the cul de sacs, and this disparity must rapidly increase from trial to trial. Let X be a blind alley, interpolated between two segments of the true path, A and B. Let the animal be running forward through segment A leading to B and X. Since the probabilities of entering B or X are even in

the long run, and *X* can be entered on the forward journey only through *A*, it follows that *A* will tend to be traversed twice as frequently as *X*. What is true for *A* and *X* will hold for any segment and its succeeding cul de sac on the forward journey. The same relation will also apply for any returns toward the entrance box. In the latter case, however, the ratio of frequency in favor of the true path will be increased owing to the fact that an animal in these returns very rarely leaves the true path.

The usual distinction between the true and the false paths in a maze is wholly meaningless from the standpoint of a learner, be he animal or human. Such a distinction can arise only as the maze is mastered. From the standpoint of the immediate sensori-motor situation in which the animal is placed, the true path and the cul de sacs are to be distinguished from each other on the basis of the degree to which they impede or encourage the animal's activity. A blind alley is but a sensory obstacle or impediment to the animal's activity; it means hesitation, caution, investigation, or disastrous sensory consequences. The true path presents fewer obstacles; it offers greater encouragement to freedom, continuity, rapidity, and vigor of motor expression. The difference is merely one of *degree*. The blinds check, thwart, and suppress activity more than does the true path, while the latter encourages and facilitates activity more than does a blind alley. The principle of relative intensity is here effective; acts are selected or eliminated according to whether the sensory consequences tend to facilitate and intensify them on the one hand, or to disrupt and suppress them on the other.

For the third problem, let us assume that an animal is reacting positively to some food object sensed at a distance. Let the conditions be so arranged that any contact with the object will result in an electric shock. The animal thus comes to the problem endowed with two ready-made connections, a positive response to the object, and an avoiding reaction to the pain stimulus. We shall term these connections *S–M*, and *P–A* respectively. The functioning of the first tendency inevitably results in the arousal of the second and the two are

brought into conflict. This second disposition is necessarily the stronger or it would not dominate in the conflict. In this situation, the second act *A* becomes connected with some sensory aspect *X* of the object, and the new connection *X–A* is formed. The only essential requirement of this stimulus *X* is that it be sensed at a distance. The further task of learning now consists in the development of *X–A* to a point where its functional efficiency is greater than that of *S–M*. Its strength does not need to exceed that of *P–A*, because the successful functioning of *X–A* will prevent the animal from receiving the pain stimulus. All three of our principles favor the connection *X–A* over that of *S–M*. The avoiding response is the more recent. The retentive development of the positive response is interfered with by this violent inruption of the pain stimulus and its motor results. The latter disposition is, however, subjected to no such distractions. The negative response is the more intense and vigorous one. Because of its greater recency and intensity, the resulting disposition is so susceptible that it is likely to be aroused and become connected with almost any opportune stimulus connected with the total situation. What this stimulus shall be and the number of trials necessary to establish an efficient connection will depend upon the nature and intensity of the possible stimuli and the organism's capacity to sense them. After the new connection approximates the old in functional strength, the factor of frequency will become operative and assist in the final stages of the elimination.

In considering the efficiency of these principles, possible critics must consider that they can work in coöperation, and that their effects are also cumulative from trial to trial. Neither does it follow that this cumulative effect proceeds according to an arithmetical progression. Neither do we contend that these are the *only* possible principles; the theory makes no pretense of completeness and self-sufficiency. These principles are also designed to explain the *mere fact of selection*, why an act with certain attributes invariably survives and why all other acts are eliminated or suppressed. There is no pretense of an explanation of *all* aspects of the learning process.

We emphasize this point because we wish to avoid the charge that our set of principles is obviously incomplete and inadequate to an explanation of the learning process as a whole. To illustrate, it is known that the 'temporal distribution of the trials' is a very important condition of learning in both the human and animal realms. But this factor influences merely the *rate* of selection; it determines in no way *what* acts are selected and *what* acts are to be eliminated.

Our conception may be briefly compared with the algedonic theory in several respects. The algedonic theory generally regards pleasure and unpleasantness as the sole principles of selection. We deny this adequacy in both the human and animal realm; we deny the assumption that all eliminated acts are unpleasant and that all surviving acts are pleasant. Such a conception does not allow for the existence of neutrally toned activities. Many useless and trivial acts are frequently selected. Acts both useful and useless may become fixed and the learner may remain in blissful ignorance of their very existence, to say nothing of being aware of their affective character. Many acts are learned which are distinctly unpleasant during the early stages at least. Pleasant activities often fail to reach the habit stage. With our conception these anomalous cases are readily explicable.

In the animal field with which we are specifically concerned, the algedonic theory meets the difficulty of an objective criterion of these subjective conditions. Obviously these selective factors must be validly inferred from certain aspects of the objective sensori-motor situation. Some discussions seem to ignore the issue. Others apparently commit the fallacy of utilizing the phenomenon of selection itself as the criterion of pleasure. This is a case of reasoning within a circle. The selection of a certain act is observed and noted. The conclusion is reached that this act was pleasurable because it was selected, and after the existence of pleasure is thus determined, the further conclusion is advanced that the act was selected because it was pleasurable. Such an explanation is merely verbal and any alleged knowledge obtained is purely chimerical. Any valid algedonic theory must choose as its criterion of

pleasure some fact or aspect concerned with the learning process other than the fact of selection itself. Moreover, this fact or aspect which is to be utilized as the guarantee of the existence of pleasure must be one which can be observed, studied and described without any reference whatsoever to the fact of selection. The difficulty of determining such an objective factor and of securing any general agreement upon its validity is readily apparent. Moreover, the law of parsimony would suggest that this objective factor be utilized as the selective agency itself rather than as a mere index of the causal principle.

Any theory is in duty bound to attempt some rational connection between its explanatory principles and the phenomenon of selection. The algedonic theory meets difficulty in this respect. The affective processes must be so conceived that they can exert some causal influence upon the sensori-motor activities involved. We do not contend that this difficulty is necessarily insuperable. We merely refer to the fact that no conception has won any general acceptance. Moreover, the algedonic theory necessitates a double mechanism. Pleasure must increase retentive development, while unpleasantness must check and decrease it. Elimination is thus not a mere matter of suppression but a tendency which operates to tear out connections by the root. In opposition to these difficulties, our conception offers a single mechanism for both selection and elimination, and one which is in harmony with generally accepted principles of psychology.

Supporters of the algedonic theory may admit our positive contentions, but urge that there is no necessary antagonism between the two views, that the two can and should be combined. Selection would thus be determined by the recency, frequency and intensity of the pleasure. Such a combination seems plausible at first thought, but a careful analysis will show a combination is impossible. Either the affective processes are the sole selective principles and the objective factors play the subordinate rôle of determining the *rate* of selection, or the objective factors are the real selective agents while the affective elements are mere useless luxuries having no function whatsoever. The situation offers no opportunity for the straddler.

CERTAIN FURTHER FACTORS IN THE PHYSIOLOGY OF EUPHORIA[1]

BY GEORGE V. N. DEARBORN

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I. Introduction

In a recent article (5) on the relations of sthenia to certain phases of education, the writer had occasion to mention, and a little more, some of the factors of physiological euphoria (experience of satisfaction or of pleasantness) as set forth, for example, by H. Spencer (25), Marshall (20), Max Meyer (21, 22), myself (9) and many others early and of late. The essence of all such discussions seems to be the ample, unimpeded, undeflected, and furthering nervous impulses from large receptive fields especially such of these as represent personal expansiveness. It is our present endeavor to make slightly more explicit certain of these receptive fields, loci of personal relation to the effective environment.

One matter of definition of terms must be understood at the outset, as all along: "euphoria" is not necessarily coextensive with happiness or even with "the experience of satisfaction or of pleasantness" for either of these may be, and of course frequently is, dependent on ideational processes

[1] From the twenty-second annual meeting of the American Psychological Association at New Haven, 31 January, 1914.

proper, in the narrow sense, that is, of conceptualization: a mere idea, a pure idea may make a man, for the time at least, happy and afford him what we might well learn to call *ideational* euphoria as in some degree different, especially in its occasion, from the euphoria of which we write—physiological euphoria. It should "go" without being said that in the long run the former has dependence on the latter; else, indeed, all of us more or less were poets, artists, metaphysicians, reformers, and science had no place. In choosing a home-site a large library or even a prospect sublime as the skies is not enough. Our concern in this immediate article is then with euphoria in its dynamic or physiologic relations. We shall discuss some of the bodily and environmental conditions that seem most importantly connected in and with physiologic euphoria under normally biologic circumstances, the interesting euphoria of insanity being, for the time, ignored. Advancing understanding of certain physiologic relations makes further psychologic analysis now possible, and provides us with more cenesthetic "mind-(?)stuff" as the receptive fields become better and better understood.

It is a traditional presumption worthy of all acceptance that the affect, the emotional balance whether euphoric, dysphoric, or neutral represents the personal reaction when the individual is more or less well adapted to its environment, especially its organic environment. The individual as a whole and neurally has some "attitude" toward non-individual part of his experience. It is important to note that for neurology a part of this objective environment may be within the body of the individual, anywhere in short except within a nerve-center! For convenience, then, in this particular discussion, and perhaps elsewhere, we may divide a man's environment into two parts equally essential almost to his behavior and to his well-being, 1, outside the skin altogether and, 2, outside the receptors in the viscera, muscles or wherenot in the body. So far as neurility is concerned obviously these are equally environmental. (Such a consideration is only one more step in that integration of life and mind and environment which is a key-note of our day.) That in the long run and in general the affect

represents more truly than anything else the personal reaction to environment thus broadly denoted need not be enlarged upon here. Verworn in his Yale lectures published under the title "Irritability" (**26**) has recently made an elaborate analysis of the bases of this interdependence and concludes, rightly and usefully it appears, that "a stimulus is any change in the vital conditions." This is a good step beyond Fechner and Du Bois-Reymond and in the right direction of orienting personality and its determinants.

We seek, therefore, to make a bit more definite and concrete the current notions as to the euphoric receptors as receivers of changes in the vital conditions and to suggest, if we may, the general nature of the afferent influences that determine the tonicity or strain or neurokinesis or whatever in the central and especially in the cerebral, gray. Such a discussion is in a sense and degree the homologue on the one hand of a sketch of the neurology of voluntary movement already proffered (**11**) and, on the other, of the neurology of the vegetative mechanism to come as soon as the autonomic system, especially its afferent aspects, now so blind, shall have been revealed. Thus little by little behavior's neurility will be elaborated as a truly adequate "sanction of psychology."

II. Possible Further Euphoric Factors

It appears that three factors especially of the influences of the euphoric receptive fields may be profitably described in somewhat new detail. Each of these is complex enough and as yet little enough understood to challenge the curiosity and the scientific constructive imagination of many investigators. The three factors noted are: *A*, nutritional and sympathetic influences from the active intestinal villi; *B*, kinesthesia; and, *C*, the epicritic impulses. Let us examine into the part each of these obvious factors takes in euphoria; it seems to be another troublesome case of estimating our income of life-satisfaction at the source. It is clear that ideational euphoria and the ideational elements of euphoria in general arise more directly from the cortical associations, yet these latter, too, recent observations suggest, secure their animus in the environ-

ment to some extent and largely in the innervation of expression-muscle be the view that of animism or other.

A. The nutritional and sympathetic *influences from the intestinal villi* are clearly two things and not one. The nutritional influences toward good humor, "feeling good," go to the neurones (and especially to the cortical nerve-cells?) in the portal and systematic blood-streams; while the sympathetic impulses are the little-known but certain afferent autonomic nerve-currents which experimental physiology and one's personal experience both suggest. We may dismiss these neural contributions to the cenesthesia of comfort and well-being with the above word,—but of necessity, not from choice, for it may not be doubted that these afferent impulses from the viscera have much of importance to do with the determination of moods and passions and temperaments, with, in short, all the most fundamental affective themes that underlie consciousness and behavior.

The nutritional influences toward good humor are not as yet on a basis of physiological demonstration through vivisectional experiment and chemical analysis of the blood in euphoria and in dysphoria. Such demonstration would be difficult to carry out owing largely to our necessary ignorance of the affective tone of a brute when not part of a definite emotion such as fear or love. The affect during a fast deliberately carried on for days or even a few weeks is a problem apart, and one which merits study: in some manner, much perhaps as in a bad pneumonia or other asthenic fever, the weakness here merges into a relative anesthesia in which the affective tones both ways are indistinct. But under ordinary conditions of nutrition there is undeniably, I think, a demonstrable direct relationship between absorption from the small intestine and the affective tone. The dysphoria of acute fatigue, in part certainly central, would otherwise not be so immediately relieved by a glass of hot milk or malted milk or of some variety of soup. A hungry worried man home from the office to dinner could not feel his dysphoric worries slip away so very quickly from any other influence than a direct nutritive stimulation of the central, especially cortical, nerve-cells. It is not a traditional delusion

that fat men and boys are usually good-natured and lean women cuttingly keen and not obviously too happy. On the one hand the Esquimaux and on the other the races of southern Europe, both eaters of much fat, certainly have a higher euphoric index than the Scotchman, for example, or the thin down-East Yankee.

While the exact chemical nature of the Nissl's granules (tigroid substance, chromatophile bodies, chromatin bodies) is not definitely known as yet (owing to the minuteness of the nerve cells and the impossibility so far of separating thees bodies from the cytoplasm around them), there is general belief that this material chromatin is a complex substance essentially compounded of fat and protein in which the characteristic determinant is what the biologists term a lipoid, a "fat-like" material, phosphorized fat (Goldscheider). Austin and Sloan (1*A*) and Dolley (12) have demonstrated the direct dependence of nerve-cell activity on the chromatin masses, and the quick loss of chromatin when the katabolism exceeds the anabolism. I think we may, then, deem the cerebral end of the process under discussion well established:—that the normal action of the cortical-neurones requires an abundance of circulating fat (or "lipoid"-producing carbohydrate) as well as of protein. In addition to the chromatin-masses, the myelin sheath of the medullated nerves is an adipose substance, very likely nutritive to the neurite.

So far as the competency of the circulation is concerned to keep the minute nerve-cells in immediate and constant relation with the blood-plasma (lymph), it is enough to remind the reader that a blood corpuscle passes entirely through the systematic and the pulmonary "circulations" in about thirty seconds; that the capillaries are everywhere and with walls so permeable that they exist, so to say, only for the red corpuscles; and that the protoplasm of the human cortex is about 85 per cent. water. The temporal unification then of nerve-cell nutrition and the portal blood (from the intestine) is surprisingly complete; an increase in the fat-content in the thoracic duct would be almost immediately used in the cortex, raising the metabolic tonus of the nerve-cells to a better affective concomitance.

Passing now to the mechanism and process of intestinal absorption we see in the neuro-musculo-glandular mechanism of the villi and the valvulæ conniventes a much more competent and actively adaptable apparatus than has been disclosed heretofore. In general terms the presence of muscle means the active adaptation of an organ to conditions outside itself, for example in the absorptive organs mentioned as well as in the spleen and ovary.

Whatever may be the adaptive movements of the valvulæ, the villi are the chief immediate organs of food-absorption from the intestine. There are about four million of these organs in the human. They are irregular but in general finger-shaped organs varying in length from 0.5 to 3.0 millimeters. Their combined surface area situated as they are on the valvulæ conniventes increases the absorptive area of the intestine at least an hundred fold over what it would be were the gut a smooth-walled tube instead of one partly filled at times by these organs. The villus is a complex little organ for, besides its versatile and essential wall of columnar epithelium, it consists of smooth muscle, autonomic nerves, a conspicuous central lymphatic ("lacteal"), blood-vessels, leucocytes, and connective-tissue. It would be pedantic to venture conjecture as to the exact modus operandi of such a mechanism. Fat, however, is the only alimentary principle which is mechanically conveyed from the midst of the epithelial cells (where it is apparently synthesized from the fatty acids and glycerin) to the central lymphatic. The chief function of this "lacteal," so far as known, is to receive the fat globules and to forward them into the circulation proper via the thoracic duct. Howell says (1913): "The mechanism of absorption remains unexplained." It is, however, extremely probable that the neuromuscular mechanism of the villus has, as part of its function at least, the compression of the villus under nervous requisition for more fat from other parts of the body. On this basis the villus is understandable as in part a minute reservoir of adipose material, perhaps indeed chiefly for the greatly variable use of the nervous system, nerve-cells (chromatin) and nerve-fibers (myelin) alike. They clearly make up by their number what each lacks in size.

We have now before us the vitals of a mechanism, receptors, adjustors, and effectors all complete, by which the nervous system may secure fatty (and protein?) nutriment suitable to its wide range of requirements in rest and activity and dynamogeny.

It may well be taken practically as an axiom for the relations of mind and body that optimum physiologic and psychologic function represents, in the long run at least, physiologic euphoria; at any rate we are basing this metabolic suggestion on this presumption, and probably without protest.

B. Kinesthesia is the second kind of contribution to be noted to the neurility of physiologic euphoria. The status of this, the fundamental behavior-sense, to so crudely term it, has been of late set forth by the writer both in its physiologic (**7**) and its efficiency aspects (**8, 11**). Suffice it then to say here that thousands of afferent impulses, strains, or influences from as many receptors in the joints, muscles, tendons, skin, and bones are continually pouring into the psychomotor centers. These represent in the ultimate analysis the *environment* to the personality within and, more specifically, integrate the body and the mind, furnishing to the psychomotor centers their only data by which the body may be coördinated.

From the somewhat different view which is rapidly becoming understood and accepted (like subconsciousness and the new-mind-stuff theory and the expedient continuity between protoplasm and mentality), usually denoted as the reservoir-idea, kinesthesia supplies a considerable fraction of the intake of this head of neural energy. Muscle of course always, even in deepest slumber, has some tonus and always therefore, together with its mechanical fellow-tissues just mentioned, is sending floods of energy into the central nervous system from the latter's environment. This gray-reservoir idea has not as yet been universally enough accepted by psychologists; yet nothing is more certain than that in some mode it exists, and its usefulness to psychology every man may read.

The precise relationship between euphoria of the physiologic type and the quantity of this kinesthetic flood representing the body is probably not a direct relationship at all, for else

conditions of the greatest bodily activity (short of cramp or catalepsy or general convulsions) would be those of the greatest delight (in so far as affect and not sensory pleasure). This of course is not the case in a bald and strict sense. On the other hand, the present writer believes that it is more nearly true than would be generally supposed at first glance and that the observed discrepancy can be explained by extraneous circumstances such as distraction of attention without, etc. In certain forms of general activity represented by kinesthesia to the brain, the relation of the quantity of movement to the euphoric index is obvious; in other phases of neuromuscular activity the direct correspondence cannot be directly made out.

Ribot in his latest book (*22B*) has set forth in a most interesting and important way the utter dependence of the subconscious aspect of mind on kinesthesia. And the neural determinants of euphoria are of course inherently subconscious. (This book of Ribot's, like his others, should not be overlooked by any one who would understand the bases of behavior, for they are as fundamental as they were prophetic in modern psychology.)

On the whole, so far as the mere amount of euphoric kinesthesia is concerned the correspondence seems certain but often vague.

In bodily activity in which the kinesthesia is intensively conscious and widespread the direct relation appears in emphatic form. This condition of intensive inhibitory consciousness we commonly designate as grace or gracefulness, and it is clearly a generalized skill. The logical limit of grace in the human active body would be in a form of exercise forever denied to man, namely flying. Swimming, skating and real (classic) dancing appear in practice as the maxima of grace, then, and these, as arranged here in an ascending series, seem to be, other things equal, conditions of physiologic euphoria.

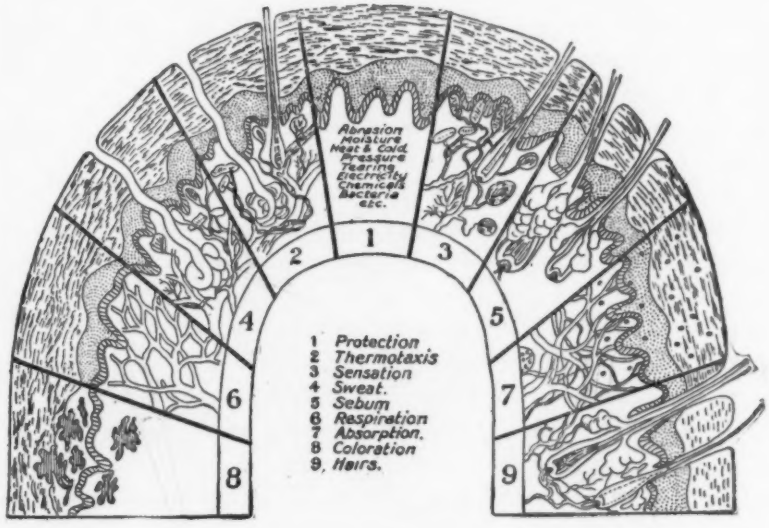
When (to take a third case and phase) the kinesthesia is localized but all the more highly intensive and inhibitory and conscious on that account, we have a kinesthetic euphoria arising in any form of what is commonly termed skill. A sleight-of-hand performance, guiding a fret saw, engraving on

metal or carving wood, drawing, pitching skillfully a base-ball —all such movements have an inherent pleasantness essentially distinct from the other factors which take part in the total behavior. They supply in intensity of kinesthesia what they lack in quantity of ingoing stimulation. From this kinesthesia comes in part the euphoria in general of all creative motor processes; without it not even the one-in-a-million child would ever learn to play the violin within the limits of human toleration.

Ample, unimpeded, undeflected, and furthering nervous impulses, then, from the action system seem to be an important factor in the euphoric neurility whenever the neurones are adequately nourished.

But kinesthesia has more to say in this matter, probably, than to emphasize *itself!* Bergsonite at least to this extent, I am convinced that in as much, in so far, as kinesthesia does, as already suggested, represent the environment to the personality, every sense owes its experience of intensity and of extensity to kinesthesia. Sensation proper has only quality it really seems, and whatever of quantity or intensity it represents in experience or in fact it derives from the spatial (bodily) aspect of our two-aspected personality. It is bodily reaction (as consciousness, what but kinesthesia?) which supplies the extensity, as well as the intensity, to sensation. Years before the French light [Bergson and (22*B*)] dawned upon my physiologic ken, the absolute necessity of recognizing how inherently implicated space (and all its relation) in our personality, was made explicit in certain extravasations on voluntary movement (11). The body represents environmental space (as already defined) just as kinesthesia represents body and physiologic euphoria represents kinesthesia. In this nexus of inevitable experience only does the soul of man get its insistent purposes revealed—whether fulfilled or not, at any rate revealed; only so, then, does it reach reality and self-hood. Of this personality euphoria assuredly is an important aspect. The far-reaching meaning of this relation between space and experience has not as yet been adequately applied to the becoming science of behavior, the science of "things as they are."

C. The third suggested factor in the euphoric cenesthesia, noted above as *the epicritic impulses*, is even less readily described with scientific satisfaction because, like kinesthesia, the human skin is a relatively neglected field for intensive study by the histologists and physiologists. It appears that stains more clearly differential than those now in use are needed before the anatomic basis of the epicritic influences and the many factors of this complex and important organ can be made fully known. Few save certain specialists in biology realize how complex this simple-looking mechanism really is.



The accompanying diagram, from the writer's textbook of physiology (4), shows the various functions, but, like all other pictures at hand, fails to suggest adequately the varieties of receptors,—in this case the chief interest.

A list of the so-far-discovered more or less influential elements of the human skin would include the complex vasomotor mechanism, afferent sympathetic dendrites, the peculiarly efficient epidermis, sweat-glands, sebacious glands; Meissner's corpuscles, the terminal cylinders of Ruffini, tactile menisci, the nerve-rings of Bonnet (?), Vater-Pacinian corpuscles, free nerve-endings, heat-receptors, cold-receptors, pain-receptors,

pleasure-receptors, perhaps tickle-receptors; arrectores pilorum muscles with their coördinating nerves.

The pioneer uncoverers of all these things (Goldscheider, von Frey, etc.) we do not for our present purpose need to refer to, but certain recent observers and writers have emphasized the completeness of the *integration* of these dermal functions. Thermotactic physiology has long realized one phase of this unification, but other phases have been more recently developed by Head, Sherren, and Rivers (**15**, **16**), and L. Hill, etc. (**18**), Sewall (**23**), Crile (**3**), and others. The almost heroic work of Head in this direction is a fine contribution to our knowledge of dermal cenesthesia but important as it is for psychologic physiology has less immediate use for us now than the suggestive papers of Sewall and of Crile combined with other inductions of those hygienists who are working on the science really underlying ideal ventilation.

For one thing, evidence accumulates that one large and well-nigh indispensible element of euphoria is stimulation of the skin in the way primevally *natural to it*, and in such a way as to allow of the free action of its combined receptive fields. The dermal receptors appear to be tuned to act euphorically, that is normally, only together, not separately nor in generic groups. The fields in short of the dermal sense organs are really receptive *fields*. Just as a spring wind blowing over a rich natural meadow would beneficially influence all the different kinds of herbage at once to the general enrichment of the field, making all of it glad, so, somewhat, here. The simile indeed is more than a chance choice, for it is to just this matter of moving air, outdoor air even when indoors, which I would call attention now as a basal factor in physiologic euphoria.

The empirical facts, at least, become more obvious if we state the matter in its negative aspect: We all realize that air that is "dead" (*i. e.*, not moving) (Hill, *loc. cit.*); humid and too warm; humid and too cold; or lacking in oxygen; is a ready occasioner of organic dysphoria. We are here not concerned with pathological conditions, but it is relevant at least that the still blind complex we designate as chill in some way dominates the skin's euphoric balance and indeed that of the

entire cenesthesia or sensation-fabric. Ill-defined irritations in the abdominal or thoracic viscera (especially it seems in the lungs and the intestine), and a rapidly rising body-temperature from whatever cause, almost at once can change a distinct euphoria to dysphoria at once as deep-seated and as superficial as receptors are to be found, but dominant especially of the dermal affect. It would be pedantic to claim this ill-comprehended state of chill as neurologically a lack or a set of lacks, but to suppose the normal conditions mentioned at the beginning of this paragraph lacks seems wholly justified by over increasing real knowledge of the relations of the skin to its environment. Neurally then, dead air means a lack of movement over the skin; air that is humid and too warm means a lack of stimulation by the optimum temperature and by evaporation; air that is humid and too cold means similarly a lack of the optimum temperature and a lack of dryness, evaporation so being hindered with its own important neural lack; lack of oxygen in the air, whether from its general chemical composition or from its utter deadness next to the skin, means an obvious lack of receptorial stimulation that we shall briefly consider anon. The whole trend of recent opinion, notably Sherrington's (**24**), is to suggest how minute, both qualitatively and quantitatively, is the adaptation under different circumstances of the receptors, especially those of the cenesthesia. Indeed Sherrington's conjectures along this path have opened a first way for useful working hypothesis on which to proceed as to the viatility of the spinal gray. Lack of movement, optimum temperature, dryness, and oxygen certainly mean lack of stimuli for very numerous receptors and a consequent lowering of the cenesthetic tide. Neurologically they seem to represent the absence or reduction of multitudes of neurokinetic contributions to the tonus of the areas of the cerebral gray, and therefore a diminution of the environmental energy representing conscious well-being. The "head" or pressure in the dynamic "reservoir" is lowered or alienated or opposed and normal euphoria thwarted.

The careful tuning, so to say, of the many and many kinds of receptors, is a matter worthy of extended research in

the laboratories. It has been already virtually discussed to a slight extent in descriptions of the heat- and cold-sensations and in the conjectures as to their continuity or discreteness, etc. Indeed these particular receptors show what I have called tuning in an especially clear way. Evidence accumulates, however, that other sense-organs, those of oxidation, of evaporation, of tickle perhaps or of touch, may be in like manner if not in like degree relative to extraneous conditions and—tunable. It must be remembered that the dermal senses, like smell, in the human are distinctly decadent senses (save in the fingertips, tongue, etc.), because so unnaturally (from a biologic standpoint) protected from wind and cold and sleet and storm, from especially perhaps the oxidation, local and reflex, for which these environmental modes of energy stand in the central nervous system. The conscious aspects of these receptors have apparently degenerated in the centuries that have gone, but the neural influence, especially in so fundamental a relation as that of the algodonic balance, remains unchanged.

Says Ellis (13) "The hygienic value of nakedness is indicated by the robust health of the savages throughout the world who go naked. The vigor of the Irish, also, has been connected with the fact that (as Fynes Moryson's *Itinerary* shows) both sexes, even among persons of high social class, were accustomed to go naked except for a mantle, especially in more remote parts of the country, as late as the seventeenth century. Wherever primitive races abandon nakedness for clothing, at once the tendency to disease, mortality, and degeneracy notably increases, though it must be remembered that the use of clothing is commonly accompanied by the introduction of other bad habits. 'Nakedness is the only condition universal among vigorous and healthy savages; at every other point perhaps they differ,' remarks Frederick Boyle in a paper ('Savages and Clothes,' *Monthly Review*, Sept., 1905), in which he brings together much evidence concerning the hygienic advantages of the natural human state in which man is 'all face.'" Of these 'hygienic advantages' euphoria certainly takes no small part. Says Sewall (23): "It is through the genius of the moving air of the open that the temperature

nerves find their most salutary stimulus and induce metabolisms characteristic of highest machine efficiency in the body. The physiologic difference between 'open' and 'closed' air depends partly or wholly on differences in the stimulation of the temperature nerves of the skin under the two conditions. The environment of the open air is conducive to an esthetic state that should not be ignored as an aid to healthy living." Indolence has been preponderatingly active among the savages of the tropics, but who can doubt that herein is one of the factors in the predominance of the man and the woman in the earth's temperate zones where the air is cool and stimulating, helping to make them happy as well as healthy,—closely related states.

A biological consideration is worth suggesting: The skin was the primeval receptive field in the phylogeny as still in the earliest months of the ontogeny, so that subconsciousness relates gentle stimulation, especially of optimum temperature, gentle friction in particular, to euphoric states. The reader does not need to be reminded that every known animal of sufficient evolutionary grade acts as if it enjoyed gentle massage of its skin. Closely related is sexual "contrectation" (Moll) which explains the seductiveness of the caress in its relation to the euphoria of being loved—the psychophysiology of which the writer has recently developed for the School of Eugenics.

The matter of the pleasantness arising from the movement of the environmental air even when a human body is clothed as is customary in English laboratories is vividly illustrated by Hill (**18**): Eight men were confined in three cubic meters of space; the temperature was 87° F., the content of carbon-dioxide 5.26 per cent., and of oxygen 15.1 per cent. "The discomfort felt was great, all were wet with sweat, and the skin of all was flushed. The talking and laughing of the occupants had gradually become less and then wholly ceased [dysphoria]. *On starting up the electric fans* the relief was immediate and very great," (euphoria) in spite of the rising temperature. No headaches followed or other after-effects. In another experiment carbon dioxide up to 2 per cent. was forced into the box unknown to the eight subjects without being noticed at all by them. Most of us have had essentially

similar experiences on sultry July days when a breeze has suddenly sprung up. The pleasantness then is a very real and physiological experience.

Professor Wm. G. Anderson, of Yale, has recently published (1) the report of a virtual repetition of Hill's work (18), and is inclined to conclude that a great increase in the proportion of carbon dioxide (from 20,000 to 30,000 per cent. above the outdoor normal of about three parts per ten thousand) is an important feature in the causation of the distressing features of a lack of ventilation. How could it be otherwise? The subjective reports of conditions as experienced or felt suggest that the feeling of dyspnea is distinct from that arising in the skin, for the starting of the fans in the close chamber afforded relief but the mild feeling of suffocation was not lessened.

One subject (a medical sophomore) reports "slight headache relieved by the fan"; another: "feeling of warmth and perspiration which was relieved by the fan;" another: "fan turned on, felt cooler and headache disappeared;" another: "discomfort, which was slightly relieved after the fan started"; another subject, the experimenter, "No symptoms except slight dyspnea [but] fan relieves the feeling of heat and wetness, but not dyspnea. After these tests a slight sore throat."

In another experiment with temperature of 83° F., relative humidity 43, carbon dioxide 8.10 per cent., despite the starting of the fans which "relieved the closeness and warmth," (ten adults were in the cabinet) the suffering from the dyspnea persisted: "the sense of oppression was such that the women felt as if 'they should scream or faint' and the men were quite ready to force open the doors." For our psychologic purpose, the subjective reports of the subjects in this part of the work are perhaps worth reporting:

Dr. R.—"Dyspnea, slight headache, relief from heat when the air was agitated but no relief from dyspnea; no permanent bad after effects."

Dr. M.—"A very slight headache while in the room and for 30 minutes after, but normal the following day."

Dr. H. L. A.—"At the end of thirty minutes very much depressed, felt weak, dyspnea, headache both sides, parietal, felt crowded, mental depression. When all left the cabinet I experienced mental and physical relief. Headache lasted three-quarters of an hour."

Miss P.—"Dyspnea, headache, depression, suffered with coldness and had slight chills for half an hour after leaving the cabinet. All right in the evening and next day."

Dr. B.—"First noticed warmth, then dyspnea and flushing of the face, throbbing of the temple arteries later, distress in epigastrium, headache which became worse on leaving the room and lasted all day. Also slight dizziness and slight mental impairment."

Dr. H.—"Frontal headache, flushed face, some perspiration, symptoms of a cold increased. Following day no bad results in evidence."

Dr. S.—"Perspiration profuse while in the cabinet, chilly afterwards, dyspnea after about five minutes, sick in the stomach, headache, micturition frequent for four hours. All rest of the day, evening and night head felt full, could not use microscope. Felt well next day when out doors but 'stuffish' in the laboratory."

Miss D.—"Had chills and other mental and physical disturbances similar to those mentioned by Miss P."

Dr. W. G. A.—"After leaving the cabinet there was severe pain in the head, temple and eyes. Some difficulty in seeing and a very evident uneasiness. Walked home, felt nervous. Ate usual light lunch. Attacked by dizziness at 1.15 p. m. (The blind headache of my boyhood.) Treated these symptoms and reported at Medical School at 2 p. m. Could not see the figures on the gas apparatus. No after headache but a heavy feeling. Vision normal at 2.30, no return of blindness, but the after effects of poisoning were present until 9.15 p. m.

"When fatigued one particular tooth aches, and certain muscles twitch—biceps, deltoid, etc.—but on this occasion the flexor pollicis twitched for ten minutes and the molar ached or was sensitive for three hours.

"When poisoned by digestive toxics I have blind headaches. When fatigued by overwork I am apprehensive and feel that much is to be accomplished that is not done. The CO_2 (?) produced this state of mind. In other words, fatigue of mind and body and CO_2 poisoning affect me alike. There was no rise of temperature and no chill. There is some slight nose-bleeding." (This subject was the experimenter.)

Professor Anderson's remark that "fatigue of mind and body and CO_2 poisoning affect me alike" is interesting especially in the light of Verworn's theory of fatigue (**26**) as essentially a suboxidation, for there is much evidence to indicate that carbon dioxide is less poisonous to man than the lack of oxygen is depressive and fatiguing. On the whole, seen broadly, Anderson's work seems to corroborate Hill's conclusions if we construe, as is needful, lack of oxygen in place of excess of carbon dioxide. In any event, Hill's findings as to the great relief in the dysphoria afforded by atmospheric movement are upheld, and no one could expect that anything at all would long compensate an organism, especially one as psychophysically complex as man's, for a deficiency of this inevitable factor of our mortal environment, oxygen.

The fact seems to be, then, in general, that more or less regardless of the composition, temperature, humidity, etc., dead air against the skin tends to be dysphoric in action; while air moving over the skin is one of the most important of all the factors of euphoric stimulation and of sthenic activity. So far as this mental aspect is concerned the temperature of the air, however, seems to be a determining quality, in the sense at least that air (or water) too cold or too hot turns the balance to dysphoria (chilliness or sultriness). Because in part of its fundamental importance in ventilation (see (**6**)) and in the science and art of clothing and in the hygiene of the bath, the dermal problem suggested here is not only a matter of large practical importance, helping to fix the conditions of our greatest practical health and efficiency, but it is also a nice enigma for solution by psychology—for the crux of the problem is psychological; to be carried out, as usual, physiologically. The present writer makes no present pretenses certainly to have solved the problem, and it is with much diffidence (sic) that he ventures the following suggestions. Real solution waits on histology or on a new kind of histologic physiology, whereupon psychology may determine the euphoric relationships.

Of the several possible explanations of the empirical facts now roughly stated above, two, taken together or possibly separately, seem the most likely in view of what is now known about the skin and its stimuli. One of these is a process of *evaporation* and the other *oxidation*. One or both of these neurally physiochemic functions seem to stand for the necessary floods of (euphoric) neurokinesis into the central nervous system.

The evaporation of the sweat poured out into the epidermis, a sponge-like reticulum of keratin, is well known to be the chief means to the regulation of temperature, in homotherms. The average daily amount appears to be about 1,500 c.c. but a group of glass-makers observed by McElroy had an average secretion of about 25,000 c.c. in the course of a nine-hour "day"; occasionally (owing to some bulbar trouble?) the production stopped, whereupon the man would become ill,

have to cease work, and would be revived by the active efforts of his fellow-workers. These facts are cited because they show the very great and useful adaptability of the sweating function and the extreme dysphoria of its cessation. Sultry and "muggy" weather shows us the same thing of course, unless we be exercising actively when in part the freer evaporation reduces the dysphoria even to its opposite, to a tone of pleasantness. One sees, for example, the students in a summer school of physical education enjoying with a real euphoria vigorous exercise with the gymnasium-temperature in the nineties.

It is not then by any means obvious that the "cold-spots" (we know nothing as yet of the actual receptors) are the sole forwarders of the streams of energy that represent euphoria due to dermal or sudoral evaporation. Several other of the receptors noted above may be concerned in this process of adaptation of the individual to his environment through energized tones of pleasantness and of unpleasantness. Here the case is homolgous, biologically speaking, to two sensations, whose end-organs are no better known, pain and pleasure.

The second process which appears to actuate dermal receptors so as to effect an euphoric tone in the individual's consciousness (not to say in his subconsciousness) is oxidation, one of metabolism's foundation-stones. Experiments done long ago seemed to show that so far as the body's respiration is concerned only about 0.5 per cent. occurs directly through the skin. But this small fraction shows that oxidative processes do occur in the skin. When one considers the minuteness of the various dermal receptors and their possibilities of actuation by the "circumambient air," together with physiologic data immediately to be noted, the reasonableness of supposing dermal oxidation to be a factor of euphoria is readily admitted. Bohr (**2**) showed that ventilation of the blood in the lungs is probably a reflex process of active secretion by the alveolar epithelium. Y. Henderson (**17**) on the other hand, while admitting the oxidative secretion, supposes that the depression of "mugginess" comes from the kolionic inhibition of this secretion in the lungs. The receptors of this oxidation reflex it is possible, or rather more, are in the skin, and may be found to be one of

the varieties of end-organ mentioned considerably above. Lusk (19) showed by experiments in which men were emersed in water at 10° C. for from seven or eight to twelve minutes that the metabolism increased 181 per cent.—and respiration is always the metabolic index. The experimenter ascribed the increase to the men's shivering, but it seems possible at least in the light of Bohr's work that the increased activity of alveolar secretion of oxygen into the blood may have something to do with the heightened oxidation.

This supposition seems strengthened by late work of Verworn (26) which demonstrates, among other important things, the immediate dependence of the action of the nervous system on oxygen,—an extension of his much earlier proof that ameba stops flowing in about an hour when oxygen is removed from its environment. Without idle speculation as to the affective tones of ameba (!), it is fairly rational to presume that some or all of the delicately complex receptors in the human skin, close to the air as they are, may have their activity and their consequent streams of neurokinesis increased by exposure to moving air as contrasted with air that is dead. It is my present hypothesis, then, that moving air in some way has a tonic action on the afferent influences from the skin by stimulation of whichever receptors in that very complex receptive field are tuned to this mode of energy. The mere presence of oxygen is not enough for a normal euphoria—it actuates, perhaps by way of the pulmonary epithelium, only when coming as a moving force (with friction perhaps) against or over the skin. If, however, friction be really an element in dermal cenesthesia, it is probably not the gross mechanic friction one is apt to think of first, but rather a subtle sort of physiologic friction, so to say, adapted to the extreme delicacy of the organic instruments so abundant in the human skin. On the other hand, the mysterious highly euphoric stimulation of a gale of wind when not outside the optimum range of temperature (as in the Nova Scotian summer-land in September) is known to all, and this implies that gross friction, friction in the ordinary physical sense of the term, may be also a factor in the experienced product. Massage and the caress seem to possibly imply the same thing.

If such be elements in the centripetal cenesthesia related to euphoria, we logically should seek to discern what becomes of the various neural influences as they enter the brain. This we cannot do as yet with perfect confidence in any further detail than has been already set forth, *e. g.*, in (5). The affective tones appear to be somehow related to the thalamus and to the great cortex especially its upper "layers" perhaps. Southard seems to suggest that there is evidence that the parietal region has especial concomitance with consciousness —a tendency to localization which seems to the present writer exceptional and in the long run misleading.

Here as everywhere in such work we are almost blocked from continuing upward and outward into the cortex by our black ignorance as to the nature of the neuronal energy, neurility. Still, here and there a step ahead is taken. For example the all-or-none principle (first applied to hearts, then by Lucas to voluntary muscle), need apparently be thought of as a law of action of the neurones also; any stimulus which actuates a neurone at all actuates it to its physiologic limit and in all its parts. This principle, if real for the nervous system, brings us willy nilly back again to the old familiar but still elusive *synapse*—the all-powerful but mythical genius which every rubber of the lamp may command at will. The process of central association concomitant to euphoria, however, remains unguessed, for the interpretation of the synapse-concept in structural terms of the material neurones is as various as its interpreters. None the less, work along the lines travelled by Sherrington, James, M. Meyer, Thorndike, Parmelee, Hollingworth, and the rest can hardly fail to lead us aright, late or soon. At present we seem capable of discussing to advantage only some phases of the afferent circuits concerned —and these, as is obvious, only with uncertain and halting tongue. This then is one opinion: that there are definite affective innervations not yet mapped out, because of the complexities and our crude and ancient methods, tied to tradition.

Another opinion has already been hinted at in our introductory sentences: It may be idle to seek viatility in Morat's sense of this useful term simply because, so far as euphoria and

dysphoria are concerned, there isn't any. Viatility, the determination of separate routes, may be confined to the "final common paths" of the attention-line (10) whether motor or ideomotor. From this position euphoria is determined (omen absit!) by an abundance of neurokinensis entering the gray fabric and by its flooding in normal amplitude and perhaps in every part the central nervous system, filling it full of happy energy.

III. Summary

1. The basal feeling-tones (euphoria and dysphoria) so far as physiological, are more or less "determined" by the environment of the receptors, euphoria representing the personal reaction when more or less perfectly adapted to its environment.

2. Three factors not yet adequately considered, contributory to the euphoric cenesthesia, appear: *A*, nutritional and sympathetic influences from the active intestinal villi; *B*, kinesthesia proper; and, *C*, the epicritic (dermal) impulses.

3. The 4,000,000 villi of the intestine, rich in smooth muscle and sympathetic nerves as well as in epithelium, probably adapt the blood's content of the nutritive lipoids and proteins to the varying immediate needs of the nerve-cells, and may besides send inward sympathetic influences which, fusing, possibly in the brain, become euphoric.

4. The tonus and the active contraction of the voluntary musculature (by way of the articular, muscular, tendinous, osseous, and dermal receptive fields) make variable but essential contributions to the dynamic "reservoir" of the central nervous system.

Moreover (Bergson) kinesthesia probably adds much of euphoric trend to both the intensity- and the extensity-aspects of all the senses.

5. The integrated epicritic impulses (from the skin and mucosæ) appear to have predominance in human physiologic euphoria, two possible means of stimulation being evaporation and oxidation.

A list of the more or less influential elements of the skin would include the complex vasomotor mechanism; the arrec-

tores pilorum muscles; afferent sympathetic dendrites, the peculiarly efficient epidermis, sweat glands, sebacious glands; Meissner's corpuscles, the terminal cylinders of Ruffini, tactile menisci, the nerve-rings of Bonnet (?), Vater-Pacinian corpuscles, free nerve-endings, heat-receptors, cold receptors, pain-receptors, pleasure-receptors, and possibly tickle-receptors.

6. Air that is dead (*i. e.*, not moving); humid and too warm; humid and too cold; or lacking in oxygen, is a chief occasion of organic dysphoria. Physiologically these conditions are lacks, —lack of movement over the skin, lack of the optimum temperature, lack of dryness (evaporation so being lessened), and lack of dermal oxygen, possible reflex determinant in part of pulmonary respiration.

As regards neural dynamics, these lacks may be deemed productive of deficiencies in the cenesthetic streams (or stresses, strains, and shears) which support the cerebral neurokinesis ("reservoir")—absence of normal stimulation from the environment.

7. Adopting for the nervous system, as we must, the all-or-none principle, the actual neurology of the euphoric and sthenic balance becomes an interpretation of the synaptic relations in the body's action-system; or, better, an idea of inundation of the central nervous system by euphoric energy.

8. Physiologic euphoria is, then, more or less determined by ample and unimpeded and undeflected neurokinensis flooding the neural gray from the kinesthetic receptors and from whichever dermal receptors represent the influence of the moving air of optimum temperature over the body whenever the cerebral neurones are not short of their lipoidal protein food.

This unimpeded flood of ample neurokinesis is the condition for a high sthenia capable of actuating or inhibiting vigorously a rapid succession of motor paths.

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A PICTORIAL COMPLETION TEST[1]

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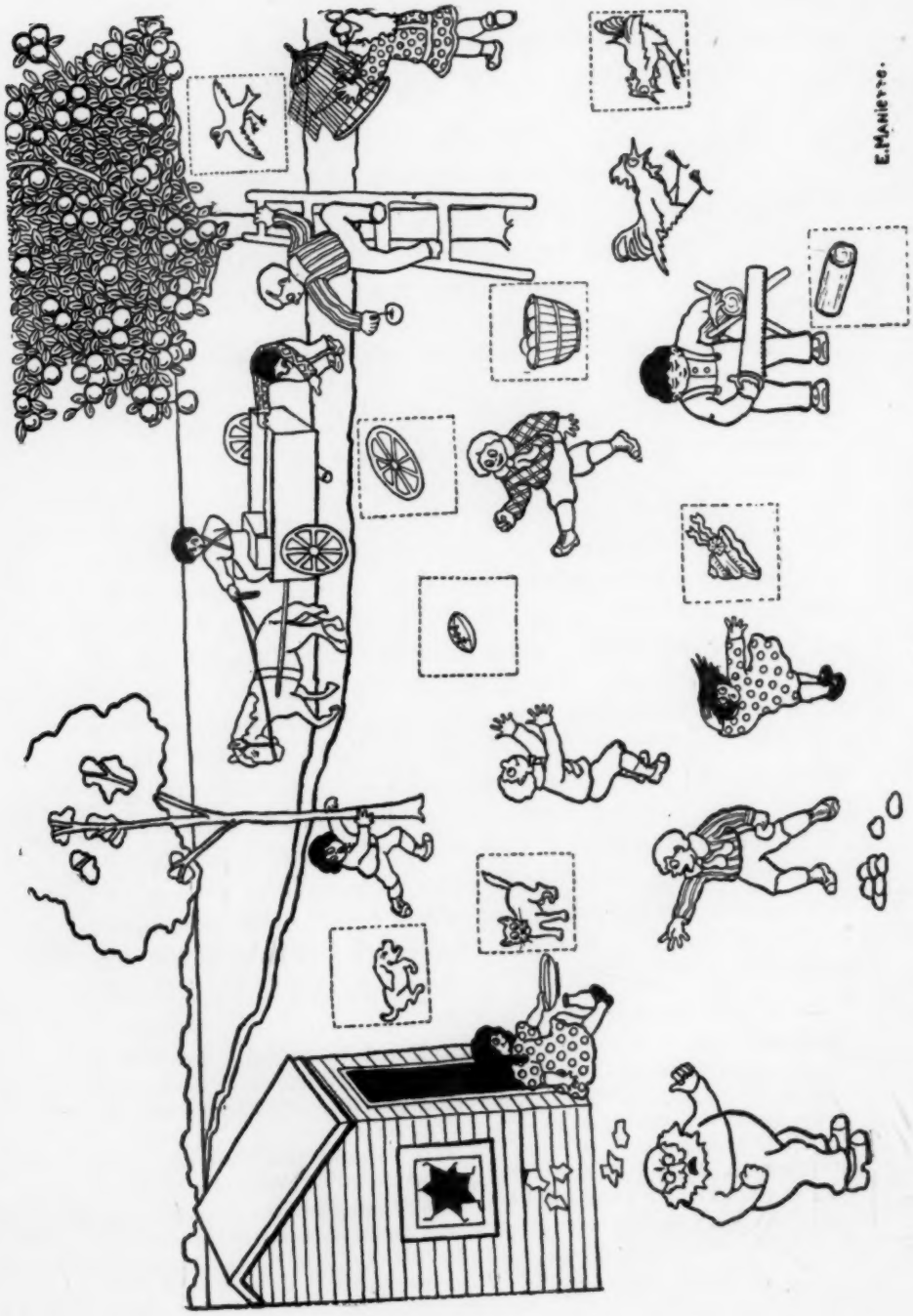
The presentation of this test has much more general and more important bearings than the establishment of its validity and norms. Several main principles for the interpretation of mental tests in general are involved.

On account of its fundamental relation to general understanding and to intelligent control of behavior, apperceptive ability is of vast importance. The wide recognition of the *Combinationsmethode* test of Ebbinghaus shows the general interest in the idea of estimating the power to make connection between different portions of the mental content. We have long realized the significant part which a measure of general apperceptive ability might play in the development of such a psychogram as we desire for our practical, clinical ends as students of the psychological factors of misconduct. But on account of the difficulties in comparing justly individuals of many grades of ability and experience in handling our own and other languages, we have been obliged to reject almost totally the Ebbinghaus method of filling in the vacant spaces of a visually presented text. The idea came to me that one might measure to some degree reagents' apperceptive abilities by asking them to fill in blank spaces where parts of the meaning of a picture had been left out. After considerable experiment a form of picture test was evolved which has been in use now for over two years, and which has received favorable comment from various users.[2]

[1] This test was first reported at the twenty-first annual meeting of the American Psychological Association. It is to be regarded as an addition to the "Tests for Practical Mental Classification" described in Psychological Monograph No. 54, issued by the Psychological Review Publishing Co., March, 1911.

[2] The gratitude of all users of this test is due the artist, Mrs. Eleanor Manierre, who through intelligent appreciation of the conditions, and by virtue of her experience in illustrating for children, was able to turn out a picture which time proves is so little open to criticism. The Wallace Press, of Chicago, showed much interest and generosity in carefully working up the lithograph.


E. MANIETTE.

The brightly colored picture, 10 × 14 inches, represents an outdoor scene with ten discrete, simple activities going on. When properly mounted (best on 3-ply scroll-saw wood), 10 one-inch squares are cut out along the dotted lines shown on the lithograph, each square bearing upon it an object from the picture necessary to complete the meaning of the separate activity. Besides these ten pieces, there are 40 other one-inch squares (produced as part of the original lithograph, but not shown in the illustration), 30 of them bearing other objects, and 10 being blank. Each piece should be so well cut out that it may fit into any one of the ten apertures; there must be no possible rejection of a choice on account of variation in form or size.

The background of the picture in general, and of each piece, with exception of the window, is all the same solid color. This involved considerable difficulty in the drawing, for it meant the obliteration of most of the perspective, and making all ideas subordinate to this essential need. Otherwise some clew other than bare meaningfulness would have indicated the proper position of the piece. We have more recently seen the wisdom of working with only 4 or 5 blanks, because these are almost never used. The picture is presented, then, with 10 apertures representing 10 places where the sense is incomplete, and with a stock of 45 ideas, as it were, to draw upon to complete the sense. The analogy to the Ebbinghaus method of completing sense in a verbal passage may at once be seen.

This test has desirable simplicity for presentation and for interpretation of results. The task is readily understood, and enlists the interest of all. The development of a *method of presentation* was not difficult. It seems most satisfactory to begin by explaining that any of the pieces will fit in any of the spaces, and that the surroundings of each aperture should be studied carefully to see what is needed to complete the sense of the picture. In order to be sure the task is understood well, one activity is taken as an example. The examiner points to the wagon group and says, "What is missing from here? What is the man looking for?" Everybody says of this, probably the easiest space to fill, "The wheel is gone." The

reagent finds and puts in the wheel, and is told to study and complete each group activity in like manner. Rapidity is suggested only at first, when it is mentioned that the task should be done rapidly as well as correctly.

The *scoring* is most simple. Each aperture is recorded by name as it is attacked, and the piece placed is also given by name. If changes are made, the previous name is crossed out and another added. Thus:

Wheel.....wheel.
Dog.....(mouse) (stool) dog.
Hat.....(purse) hat.
Log.....log.

After all the spaces are filled, the reagent is asked if he is quite satisfied with his placing of the pieces, and, if not, is urged to correct them. Time is taken when the reagent says he has finished the task and made all the corrections he desires.

Although logically out of order, we must here, for the understanding of what follows, state that errors are obviously of two kinds, rational and irrational. From a common sense view of the different interpretations of the activities, and from the demonstrated apperceptive powers of children in all groups, one can decide with minimum arbitrariness which are the few errors to be denominated as logical. The criterion is readily gained by asking for introspection. If many reagents think that the unbroken window accords well with the meaning of the window group, or that a base ball is not necessarily incongruous in the place intended for a foot ball—both arrangements showing neglect of only smaller features in the given situation—then they are not errors of the magnitude involved when the whole meaning is missed. On this basis (the common sense features of which make it unnecessary to burden the reader with the long tables of specific errors which we compiled) we find the following 10 errors to be the only ones which either are common, or are explicable by the reagent in the light of almost full appreciation of the situation.

Broken window—unbroken window. (The commonest error, but one made through not perceiving the significance of the angry man, the boy with stones, and the falling glass.

Unfortunately, some people fail to find the latter well enough illustrated.)

Foot ball—base ball. (Also a common error, but clearly one of carelessness, and readily discerned by even some of the feebleminded.)

Flying bird—standing bird, or blank. (The first is not seldom given as satisfactory, although search for a better idea easily reveals the error. The second is rare, but, of course, is quite logical.)

Chicken—mouse, or cat. (The chicken might be jumping at either of the last two. Seldom.)

Cat—cat walking away. (Of course cats are not supposed to be walking away from milk, but perhaps the girl is persuading it. Seldom.)

Hat—purse. (The wind possibly might have blown away the girl's purse. Unusual.)

Log—axe. (The axe so obviously might belong to the scene that the position of the saw is neglected. Unusual.)

Basket—baby. (Very rarely given, but when the man's action is explained as bringing down the apple to comfort the baby, certainly it is quite logical, even though unhygienic.)

But even the above errors are all avoided by naïve appreciation of the details, as will be shown in a minute. Hence we know that they should be reckoned as errors, although not of the worst class. Indeed, there is not one of the above situations, apparently logical to some reagents, but which has its fallacies detected by some high-grade feebleminded person. I suppose that with thousands of observations one could work out a numerical evaluation of the egregiousness of the different errors, but such a schedule might be logically dangerous in its application to the single case.

Norms.—In the discussion of norms I shall proceed from the performances of seven groups of individuals; (*a*) unusually bright young children; (*b*) 110 children from five grades in a well-known private school, as recorded by Miss Clara Schmitt during the progress of other testing there; (*c*) 248 cases—nearly all of them repeated offenders—seen as delinquents in our Juvenile Court work when a full round of tests was given

and careful diagnosis of mental ability or aberration made; (*d*) 15 cases selected for me at Vineland as representing the brightest feebleminded individuals there; (*e*) 95 college people, the general record of whose errors has been most kindly forwarded to me by Professor Eleanor A. McC. Gamble of Wellesley; (*f*) unselected intelligent adults; (*g*) cases of psychosis. From these we derive the following results that clearly have practical bearing upon use of the test for diagnosis.

(*a*) Young children of more than ordinary brightness do this test very well. Taking the nearest birthday as the age, as we do all through, Miss Schmitt found three children of 8 years old in the private school doing it with no errors at all—two of them in less than 4 minutes—and two more with no illogical errors. Others of us have made like observations. Anyone can get in this way most convincing proof of the validity of the test for naïve minds. Take an example of a slow-going and thoughtful little boy seen by me; 8 years of age, by Binet $10\frac{4}{5}$. He took 7′ 35″, made only three changes, studied out each situation carefully, in the meantime revealing his mental processes as follows: "I think there is something wrong there, the window is broken." "Somebody or something is chasing that boy." "Something is blowing her hair." "That cat's eyes are looking that way; I guess that would do." He left the baseball in place as his only error, but was not familiar with footballs.

No doubt there are precocious children who would give nearly a perfect record at a younger age if they have had sufficient experience with the world. We have made no attempt to find such cases.

(*b*) The following tabulation of the school group speaks for itself.

(*c*) The selected material offered from our Institute, homogeneous in the respect that all are offenders, is necessarily handled in a different way. School grades alone mean very little. We have classified according to an estimation of ability as judged by a wide range of tests, performance on school work, etc. We still follow the practical classification given with our first published tests in Psychological Monograph No. 54. It

should be understood that the grouping is only for this general run of cases seen in juvenile court work. Class *A* would undoubtedly be supernormal, even in the private school group, but probably the ordinary range of ability would be higher in such a school.

Age at Nearest Birthday.	Number of Cases	Grade	Limits of Total Errors.	Limits of Illogical Errors.	Median Total Errors.	Median Illogical Errors.	Range of Time.
8	12	2d	0–7	0–7	2	1	3′–9′
	2	3d	0–3	0–1	1½	½	
9	5	2d	0–3	0–3	2	1	1′–9′
	13	3d	0–4	0–3	1	0	
	4	4th	0–4	0–2	1½	½	
10	6	3d	0–3	0–2	½	0	2′–6′
	14	4th	0–3	0–2	1	0	
	3	5th	0–3	0–3	1	0	
11	4	4th	0–1	0–0	1	0	2′–6′
	7	5th	0–3	0–1	2	1	
12	2	4th	0–2	0–0	1	0	2′–7′
	7	5th	0–1	0–1	0	0	
	13	6th	0–3	0–1	1	0	
13	4	5th	1–3	1–2	2	1	2′–7′
	13	6th	0–6	0–4	1	0	
14	1	5th	1	0	1	0	3′
	110						

Class *A*, viz., distinctly above the ordinary in ability and information. 5 cases, ages 8 to 17. Of these only 3 made 1 total error each. The only illogical error was by the youngest, 7 yrs. 9 mos. (Binet age through 10 yrs.) Time range 3′ 17″ to 5′ 30″.

Class *B*, viz., ordinary in ability and information. 44 cases. Ages 9 to 17.

Median total error, 1. Range total errors, 0–5.
Median illogical errors, 0. Range illogical errors, 0–5.
Time: range 1′ 20″ to 6′ 50″, median about 3′ 30″.

Only two made more than 1 illogical error; the single 8-year-old made 3, and one peculiar adolescent, not however determinably aberrational, who did well on other tests, made 5 illogical mistakes. We have in fairness to include this rare

case, but probably it should be explained by some emotional upset, for which we try to be on the lookout. Individuals of 10 or 11 years are, naturally, comparatively infrequently seen as offenders. The four such cases we did study, belonging to the *B* group, made no errors at all.

Classes *C*, *D*, *E*, *F*, may, for our present purposes, all be grouped together as being what we have denominated as fair in ability, but for various reasons not grading as high as class *B*. 72 cases. Ages 9 to 18.

Median total errors, 2. Range total errors, 0–5.
Median illogical errors, 1. Range illogical errors, 0–3.
Time: range 1′ 30″ to 11′ 8″, median about 3′ 30″.

Classes *G*, *H*—both graded as poor in ability. 26 cases. Ages 10 to 17.

Median total errors, 2. Range total errors, 0–5.
Median illogical errors, 1. Range illogical errors, 0–4.
Time: range 1′ 35″ to 9′, median about 4′.

Classes *I* and *J*. Dull from physical causes, or subnormal mentally, but hardly to be denominated feebleminded. 31 cases. Ages 10 to 18.

Median total errors, 3. Range total errors, 0–7.
Median illogical errors, 2. Range illogical errors, 0–7.
Time: range 2′ 13″ to 12′ 47″, median about 4′ 30″.

Class *K*. Feebleminded—moron grade. (No imbeciles were tested.) 37 cases. Ages 10 to 18.

Median total errors, 5. Range total errors, 1–8.
Median illogical errors, 4. Range illogical errors, 0–7.
Time: range 1′ 45″ to 10′ 4″, median about 5′.

Two individuals each left one total, but that an illogical error. They were 12 yrs. and 15 yrs. old, and by Binet proved 9 and 8⅔ years respectively. Two others each left 2 total, but no illogical errors. They were 11 and 17 years old, and by Binet each proved 9 years old.

Class *M*. Psychoses. 33 cases. Of course this pathological group is not homogeneous, and figures on a norm for it are worth nothing. Results ranged all the way from no errors at all to 8 illogical errors, or to leaving no pieces in place. Of this later.

(*d*) At Vineland Superintendent Johnstone called in those who seemed to him and other officers to be in general the brightest members of their colony. 15 cases. Ages 16 to 33.

Median total errors, 5. Range total errors, 1–7.
Median illogical errors, 4. Range illogical errors, 0–7.
Time: range 3′ 15″ to 16′, median about 6′.

The best two records are as follows: A young man, considered the most intelligent male at the institution, did the test in 16′. He studied each place with very great care, made only two changes, and finally left merely one error, viz., the baseball for the football. He corrected knowingly the unbroken window. This boy was 21 years old, Binet age 10. Better still was the record of a girl, aged 16, Binet age 11. She finished in 3′ 30″, made 3 changes, including replacing the baseball by the football, but left the unbroken window as a final error.

(*e*) The Wellesley group of 95 college people shows:

Median total errors, 2. Range total errors, 0–8.

Time not calculated on the same basis as in our cases. The logical errors are not given for each case, so we can give neither median nor range, but by introducing other comparisons valuable results are obtainable.

Cases with no errors:

Wellesley	26%
Private school	30%
Delinquents, *B* group	33%
Delinquents, *A* group	40%

Percentage of total errors to pieces placed:

Wellesley	21.7%
Private school	15%
Delinquents, *B* group	10%
Delinquents, *A* group	6%

Percentage of total errors illogical:

Wellesley	64%
Private school	50%
Delinquents, *B* group	40%
Delinquents, *A* group	33%

Comment on these interesting comparisons will be made further on.

(*f*) Unselected intelligent adults. Very slight experience will show that many intelligent adults when confronted by simple mental tests show curious and unexpected reactions. They frequently do much poorer at them than children. All of us have noted this point and spoken frequently of it. A typical case would be the woman who looked over the completion picture critically, and could find no more reason for putting the hat in place than the purse, or the cat than the baby ("That girl appears to be foolish; she may be teasing the baby with the milk"), and so left both couples of pieces at the side of the empty squares. The difference was also shown when a professor of psychology left in the chicken tail down, a mistake (not counted as an error) which we have never seen a normal child tolerate. The same is true of other performance tests —capable adults becoming quite provoked because they do not do well at them. I myself made one of the worst records ever made in the mirror tracing test at a certain laboratory.

(*g*) Psychoses. The results on this test as performed by individuals with psychoses are sometimes most interesting; in other cases not at all so. Of course, some types of insane people may do such a test most brilliantly. Our attention was drawn to its possible value in this connection by the performance of a young woman whom we left for some moments, putting the test before her by way of occupation. After some time we noted that she had no piece in place. We went over the method with her again. At the end of one hour she had no single piece in place. Every piece she had placed more than once correctly, showing her good apperceptive powers, but she acted as if she could not be sure that they were correct and so would take them out again. It proved to be a marked case of *folie du doute*. The curiously slow and dreamy performance in some cases of dementia precox, and the accelerated, but erratic perceptions of some maniacal conditions, are also characteristic. We have had no means of doing more on this subject than observing bare possibilities of usefulness. From other places where the test is used, I hope there may be forthcoming a statement of findings on numbers of cases of various types.

Argument

In the above findings we perceive important correlations and differences. The performances of supernormal young children, whether as seen in our court work or outside, tally well with each other. The record of our *B* group compares closely and favorably with the private school children. The former ranges better, perhaps because some individuals of poor ability (as would be so diagnosed by the round of tests which we use in our institute work) figure in the school totals. We particularly note that individuals 10 years of age, whose demonstrated ability is good, do as well on the test as older persons of the same general mental rank.

The feebleminded group, as seen under two conditions, tally well in their extremes only. Nearly all make bad failures. It is obvious that rare individuals among them have developed the ability to apperceive such relationships as are demanded by this test, even though on other levels their mental equipment is demonstrably poor. This is as we, who observe the special abilities and disabilities of these mental defectives, as well as of normal persons, would expect. It is not to be anticipated that any single test can be evolved which will discriminate the feebleminded.

Then there are all degrees of feeblemindedness, and many of moronity, hence working up norms on a single test is almost valueless for establishing characteristics of such a widely varying group as may be scheduled morons, to say nothing about variance in special abilities. I am still looking forward to valuable psychological work being done in the study of such variations.

In the errors of the feebleminded it is often a matter of no little interest to ascertain just what the association of ideas was which led to placing of the given pieces. An example was the soliloquy of a high grade moron when he placed a clock in the field by the girl where the hat belongs and said, "That goes there. She's scared because she's late to school." Evidently her gesture and dishevelled hair were to him indicative of fright; fright might be induced by tardiness at school; the clock was a symbol of time and tardiness.

Individuals with psychoses may show very curious and characteristic performances. This is, too, as we should expect, since, after all, the criterion of insanity is behavior. Such a test which, much more clearly than the automatisms of ordinary conduct, shows the mind at work, might well bring out peculiarities of mental action.

When we come to the differences between the results of intelligent adults and of younger people, the former not only showing no increased facility of performance, but even a set of worse records, we are in the face of a phenomenon vitally important for all clinical psychologists. We have had, as mentioned above, intimations before this that the cultivated adult type of mind does not produce good scores with various tests which are satisfactorily used for younger individuals. But the records of Professor Gamble demonstrate this as nothing else has done. What is the reason for the difference? At present we have all too little knowledge of how age and cultivation affects mental performance, but one may be allowed to speculate a little on the causes of difference—especially since observation of comparative attitudes and the hearing of the adventitious remarks during performances leads us to know something of what is going on in reagents' minds.

In the first place let us say that we have frequently remarked in our work with delinquents, in testing and in other connections, the changes in attitude which characterize the passing of naïvete and the acquirement of mental sophistication. The older individual is prone to meet a simple situation with the idea that there must be something deeper back of it. We have seen this even in the presentation of our construction tests to some men of marked ability.

Or the older person assumes a critical attitude that the younger does not think of taking. This picture with its absence of perspective and its lines on the order of caricature, such as children themselves produce, was made to appeal to the simplest minds, and perhaps that creates for it a limitation. The "might be" of the adult, with his greater stock of ideas, is very rarely heard from the child, unless there is unfamiliarity with the represented situation. A marked example of this is

the chicken insert. Of course the chicken "might be" jumping at the cat, or at the bird in the cage. The greater experience of adults led them to perceive many more possibilities in the situation than the child sees. It may be this, rather than any conscious attempt at criticism, which leads the adult to go much farther than taking the picture at its barest face values. Again this difference may be observed in other tests. When asked for the Binet discrimination between a butterfly and a fly, what adult would be satisfied with answering, "One is yellow and the other is black"—a response quite sufficient for passing the test.

Then simple tests are frequently approached by the adult with an air of jocularity which may interfere both with doing rapid and careful work. Lack of ingenuousness here again is a barrier to good results.

The differences between our records and the Wellesley findings can hardly be due to difference in method of presentation, because the test is very simple, and all groups were given ample chance to correct their errors. Whether or not the college people really tried to do their best, as children do, I cannot answer. That is a question which only rarely has to be met in working with the younger groups.

It stands out clearly that if certain other less direct methods of scoring were used, as they might be in many tests, adult experience might rank high. For example, if we were to ask for all the subtle possibilities of the situation expressed by the girl with milk, in relation to the stock of ideas on the unplaced squares, one would immediately get a variety of responses from an adult which would be quite beyond the ken of children. Thus maturity would be vindicated.

As a test for mental age this completion picture seems to have as much substantial validity as most others, with this addition that it is a real test of ability in itself, for it is done very little better in after years than when first the ability is developed. From the tables of the private school group we learn that at 10 years the performance is as good as it is at 13 years. In fact, at 9 years the results are not far behind. Back of that we get a large number of bad failures. It was so

evident from the start that younger children as a rule failed that we have never worked up these negative findings. Professor Gamble sends us the general record of 15 children, average age, 8 plus. This shows that 66 per cent. of all final placements were errors, and that 87 per cent. of these were illogical. So far as time is concerned this is not markedly decreased after 10 years of age.

On account of the findings in the college group it is important to note that we observed no decrease in ability in our *B* group up to 17 and 18 years.

The time element, except when very rapid or very slow, seems to mean little. It is easily perceived that one person may deliberately adopt a slow or quick method, unless rapidity is emphasized as much as it is in other tests—and that would be undesirable here.

All through, we see illustration of the fact that interpretation of the results on mental tests has to be made by the use of wide limits, and with due regard for the different norms which exist for different ages—average records even possibly becoming poorer with adult life—and for the experience of different social groups. In testing those abilities which involve intelligence, interpretation along anything of the fine lines laid down for the use of laboratory psychophysical apparatus would lead to unjust estimation of personal capacities.

The idea of this completion method apparently is valid, but our picture may not be at all the best that can be devised for establishing norms of apperceptive powers. For older persons and for other groups of subjects a different picture or set of pictures may be worked out with more difficult or easier tasks involved, as in the Ebbinghaus texts. Perhaps the idea may prove valuable in several directions, as have our previously published performance tests, for instance, in the immigration service.

Summary

We evidently have in our completion picture a test for ability primarily adapted to the child type of mind. Every detail of the meaning has proved to be understandable even by morons. The performance of naïve individuals of ordinarily

good intelligence above 10 years of age should be better than in 5′, and not more than one "illogical" and two total errors should be made. A worse record than this should arouse suspicion of defect in mental ability.

We cannot expect a much better average performance with increasing years; indeed, such good results may not be obtained from older cultivated individuals. The use of the test has established a vitally important point for clinical psychologists, namely, that performance may be worse, according to simple methods of scoring, when adult experience is brought into play. The carrying over norms from one social or age group to another is not justifiable without actual establishment of validity.

As good a record as that of the standard given above may rarely be made by individuals who are defective according to other tests.

We see no correlation between apperceptive powers, as tested by this completion picture, and recidivism in juvenile delinquents.

The performance gives a remarkably good chance to see the mind at work. Mental control and association processes are peculiarly laid bare, and for the study of these in defectives and aberrational individuals this type of test offers much.

CANNOT PSYCHOLOGY DISPENSE WITH CONSCIOUSNESS?

BY ELIOTT PARK FROST

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In his essay: 'Does consciousness exist?', posthumously published, William James writes: "For twenty years past I have mistrusted consciousness as an entity; for seven or eight years past I have suggested its non-existence to my students. . . . It seems to me that the hour is ripe for it to be openly and universally discarded."

Such a pronouncement is welcomed, *von Anfang an*, by the Realists and Radical Empiricists in philosophy, who accept the term 'consciousness,' as name for a function of the thinking process. Such a pronouncement is equally welcome to the students of animal behavior, who read in it, however, only a confirmation of what they have long proclaimed: the non-essentiality of the concept of consciousness. Strangely enough, it is in psychology itself that a readjustment in extrinsic terms proves difficult. The self-consistent textbook of a psychology, thus objectively viewed, has yet to appear. If one is ever to be written, it can neither ignore what is commonly termed 'conscious process'; nor may it still use the psychic terminology uncorrected.

The business of our discipline is to describe, and to explain, certain phenomena. Description heretofore, has meant to talk in mental terms: of sensations; of percepts as synthesized sensations; of memory, of imagination, and the like. Explanation heretofore has meant to give the physiological correlate of these psychic processes: to speak of neurones, synapses, and final common paths. Whatever nice distinctions the instructor may make for himself, it is certain that the average student leaves his first year in psychology with the notion of two very clear-cut entities: a physical process that one can tap, detect,

measure; and a further somehow-psychic process, part experienced through introspection, part conceived of.

At the outset I show my hand. It is my personal bias, that, as data for a course in elementary psychology at least, psychic terminology with its present connotations, is confusing. I deplore the awkward straddle in which psychology is at present. We are trying to ride two horses at once, the horse of philosophy and the horse of behavior, and they are proving as mutually irreconcilable as the horses in the Platonic figure, one horse being white, and the other horse black. Let us keep to one mount or the other with some consistency.

"Whoever blots out the notion of consciousness from his list of first principles," must indeed as James says, "still provide in some way for that function's being carried on." It is my present, perhaps over-ambitious intention, to attempt an explanation of the "fact that things not only are, but (that) they get reported, are known," in a strictly physiological way, without any conscious or psychic implication whatsoever.

The term 'awareness,' emphasized by James himself, and reimpressed upon students in every psychological class-room, has always a mental, rather than a physiological connotation. To be aware is to be conscious. To prove awareness is to prove the presence of consciousness. Once establish the processes of awareness in an organism, and one demonstrates the presence of consciousness in that organism.

I beg that for a few moments, however, you will agree with me to understand the term 'awareness' in a different sense: namely, that it shall have exclusively a physiological import. So that, if you grant a friendly ear to this proposal, we shall speak of a physiological mechanism as, at times, becoming aware. And we shall test the presence of such awareness on the part of such a mechanism according to the usual criterion of behavior: does the given stimulus elicit from it a response. If, from a nervous structure, a stimulus does elicit any sort of response, in so far this shall sufficiently justify the assertion: "the organism is become aware of that stimulus." If, on the contrary, the nervous structure, upon irritation, makes no response of any nature, it must be then maintained: "the

organism is unaware," or, to use the synonym, is 'insensible.' And we shall remember that all the while the usual mental or psychic connotation of these terms is to be rigidly excluded.

With this distinction as a basis, let us investigate a simple type of behavior.

Within a dark room suddenly flash upon a nervously sensitive visual end-organ, a beam of light. If the pupillary reflex take place, as will transpire in the normal eye, it may then be affirmed that the eye-mechanism has become aware of the stimulus; and the resultant behavior, namely, the closing of the eye, is proof positive that such is the case. Such a primary reflex may occur, of course, wholly independent of any functioning on the part of higher neural centres.

Let us designate such a simple sensori-motor path as that involved in nervous response to a stimulus, as an *Alpha-arc*. The term alpha-arc shall then characterize any simple, single, sensori-motor path, initiated by some peripheral stimulus, and resultant in some end-effect. For an alpha-arc to function it is merely necessary that stimulus elicit nervous response.

But an alpha-arc may involve a longer circuit. If, in the dark room, the tiny flash come now from the side, it will naturally be met by a turning of the eye-ball, to bring the stimulus within the direct line of regard. Beyond structures involved in the previous illustration, there are now included at least the thalami, and the optic lobes. Still, however, we shall speak as before, shall we not, of a sensitive mechanism that has become aware of a stimulus, and has responded to that stimulus by adaptive behavior? We still have an arc, have we not, that may run its course independent of any so-called 'conscious coefficient,' and still result nevertheless, in accommodative behavior? In the lower organisms we call such behavior tropic; in human behavior we speak of it as completely habitualized or instinctive reponse.

As a matter of fact, however, whenever an alpha-arc functions so as to include the specific cortical structures, for instance, the temporal or optic lobes, we presume that there is always the further likelihood that the great complication-areas, or association areas of the cortex, be stimulated as well. When

such a further arc, aroused by an alpha-arc rather than by a peripheral stimulus, is set in function, let us denote it as a *Beta-arc*. In brief, then, when a stimulus falls upon a sensitive neural mechanism, it will normally arouse an alpha-arc, and this alpha-arc *may* in turn arouse a beta-arc; the latter being alone provoked by the alpha-arc, and presumably having for its seat the association areas of the cortex. The only experimental criterion of such a beta-arc will of course, again be behavior; but behavior significantly different from the response of the simple alpha-arc. Just what this modified type of beta-arc response involves can be shown in a moment.

In the case now before us, we speak of the beta-arc as becoming aware of an alpha-arc, in precisely the same sense that we spoke a moment ago of the alpha-arc as aware of the external stimulus, and for the same reason: it responds to stimulation, characteristically. The alpha-arc is unaware of itself, but aware of the external stimulus; the beta-arc is unaware of *itself*, but is aware of the alpha-arc. Only when and if the beta-arc in turn arouse some still subsequent arc, as a gamma-arc, can it itself become an object of awareness.

Once again, then, all simple sensori-motor arcs having their loci in the first and second levels, to use MacDougall's phrasing; aroused by stimuli peripheral to the central nervous system, and eventuating in behavior, we shall call alpha-arcs. And all arcs which are stimulated alone by alpha-arcs, having their neural seat in the association areas, and related only through intermediate structures to the end-organs, but still eventuating in characteristic behavior—these we shall call beta-arcs. Neither an alpha-arc nor a beta-arc is then aware of itself, but the beta-arc becomes aware of the alpha-arc; the alpha-arc becomes aware of the external irritating object. In a word, no arc is self-sensing, but any arc may become object for a succeeding arc. And what holds of an individual arc, holds equally true of complexes or nexuses of such arcs.

By introspection, psychology alleges that one holds for a moment as object of an awareness-process, a just-previous process occurring within one's own private nervous system. But such a prior process is commonly dubbed 'mental,' or

'psychic,' and the assertion is made that while such a process is unaware of itself, it reflects upon another mental process, contiguous, but just prior. Even for the introspectionist, that is, no process can become aware of itself. All introspection involves reflection, temporal succession. For the introspectionist there must be two processes, both alleged psychic in nature, where one process takes for its object another process, just prior.

In accordance with the tenets of ordinary psychology, then, if consciousness were cinematographic, instead of continuous, mental life would be impracticable; for while the integrity of the first process would remain unimpaired, this process could not become aware of itself, nor by hypothesis would it remain as object for a succeeding process, and hence there would exist the possibility of no awareness-experience whatsoever, whether dubbed 'conscious' or physiological. In short, if there were no memory; if no content cast its influence beyond the fleeting instant, and life were but a succession of momentary incandescences, each swallowed up in oblivion so soon as completed, then reflection, and with it introspection, would become out of the question.

Well! If no process can experience itself, be within itself both subject and object, what grounds exist for labelling any such process, 'psychic'? Is not the ascription to such processes of a psychic content, a wholly gratuitous performance? Are not the mutual relations of these arcs to be understood in simpler terms?

This the present writer believes. For him the term 'consciousness' with its psychic implicates, has long seemed to be a misnomer. Within his so-called 'world of experience,' he can find no psychic attachment. The whole conception of a mental, extra-physiological experience appears to him only as a pleasant speculation for the philosopher to play with; but one that becomes useless, if not misleading, for the teaching of psychology as a science. Is not behavior adequately explained as the resultant, in our terminology, either as the functioning of an alpha-, or as the functioning of a beta-arc, or group of such arcs? Does not this cover all that is now meant by 'conscious process'?

If we yield (perhaps to the philosopher), all 'life-rights' in the term 'consciousness,' on the ground that it is a misleading concept, we can then speak of an alpha-arc as an awareness-process, having for its biological goal the immediate adjustment of organism to stimulus. In definition of the further beta-arc process, the present writer has elsewhere suggested the term: 'consciousizing process.' The biological significance of this latter lies in its ability to take as an object (*i. e.*, to be stimulated by), prior awareness-processes, or cortical retentions of such processes (commonly called images), or both. In this way the immediate afferent impulses (alpha-arcs) are influenced by previous organic experience, and resultant behavior is correspondingly modified. In the simple awareness-processes, the nervous impulse has a fatal course to run, passing from the point of stimulation in the end-organ directly to the musculature over hereditary or canalized paths. But if the alpha-impulses in passing, set off further beta-arcs, facilitation or inhibition of one sort or another takes place, and the final behavior is modified, since there is now involved elaboration of the original impulse in terms of the past experience of the particular nervous system implicated.

There is no reason to suppose that nervous impulses differ in kind. An alpha-arc and a beta-arc differ only in that they have unlike objects. In the one case the object is a peripheral stimulus; in the other case it is a central one. If one prefer to call this awareness, on the part of one nerve path, of another nerve path, 'conscious awareness,' to distinguish it from the simple reflex type of awareness, there can be only the objection that the majority of present-day students of our discipline will feel that somehow something psychic has crept in; a conception, from our point of view, both useless and disconcerting. If we can but purify our psychological concepts of their mysterious connotations, there may be advantage in continuing to use the old-time nomenclature.

Alpha-arc awareness is after all unlike beta-arc awareness, *as a neural experience*. In speaking of the latter we might still say: 'sensation;' 'image;' 'thought;' 'mental process.' So far as these everyday terms express only a unique physio-

logical experience, different from a mere awareness experience, there is no excuse for replacing them by neologisms. But sensations will no longer now be 'first things in the way of consciousness,' but the second. There must always be at least two physiological processes, successive in time, for us with propriety to call one of them a consciousizing process, or sensation. The iris, responding to a light stimulus, can never get a sensation.

At just what point in biological evolution, beta-arcs first evolved to supplement the simple alpha-arcs, is no doubt largely a matter of speculation. They would seem to be called for, however, when the reflexes prove no longer adequate to cope with the environment, except by reference to past experience.

Past experience, retained as cortical disposition, may of course become the object of beta-arc awareness, no less than a fresh afferent path; so that the responses sent to the final common paths, to use Sherrington's terms, are the resultants of alpha-arcs, recent and remote, plus the effect of consciousizing processes upon them both.

If we are to speak of a beta-arc in function, as a 'consciousizing process,' we may speak of an alpha-arc as a 'preconsciousizing process,' or at other times, as a 'consciousized process.' The former term would characterize those alpha-arcs that are independent of beta-arcs, being originally autonomous, viz., the reflexes. And the latter term would characterize those alpha-arcs which no longer arouse beta-arcs, although they formerly may have done so, viz., habitual mechanisms. These exhibit the effects of beta-arc action, but no longer normally engender it.

Activity of an alpha-arc gives the organism, say, red-awareness. This means that the retina has responded to an ether vibration of 440 $\mu\mu$ in specific fashion: a different response from that which it makes to vibrations of, say, 790 $\mu\mu$. If this arc now an instant later arouses a consciousizing process (beta-arc) we get what we may call the 'sensation red.' Can either introspection or logic demand any further description or explanation of this 'sensation-red-experience' than to say that a

nervous impulse has passed through the cortex, and there aroused a second impulse which takes it as an object of awareness? Does the postulation of any psychic entity clarify or add to the data thus offered by physiology?

The 'intimate warmth' of which James speaks, does not present us with a Self. The 'warmth,' indeed, is explicable: the object of regard in such cases is a fresh neural path, just now in function. But to become aware of the warmth requires still a separate, subsequent, neural path, as yet unaware of itself.

So ingrained is the 'common-sense view' that physiological processes are the *vehicle* only of psychic processes which play upon and through them, that it is difficult at first to comprehend how a physiological process in function *is itself* consciousness at the moment, completely described; and that any further implication is unwarranted. But cannot psychology approach nearer to the scientific ideal of all disciplines by considering the terms 'consciousness' and 'mind' merely as handy ways for expressing the reactions or awarenesses on the part of processes beta, gamma, delta, and so on; of processes alpha, beta, and gamma, respectively just prior? Is not what is commonly meant by 'consciousness,' so far as psychology is concerned, more nearly described as a 'beta-arc functioning,' than to speak, either of 'an elementary psychic process,' or even of 'a knowing function'?

THE MECHANISM OF MENTAL PROCESSES AS REVEALED IN RECKONING

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Much laborious research has been undertaken in order to throw more light on the reproduction associations. Some psychologists have examined mainly remote memory; others, immediate memory. The reproduction of nonsense syllables, letters of the alphabet, words, numbers, etc., has been elaborately tested, by Ebbinghaus, Müller and Schumann, Müller and Pilzecker, Meringer and Mayer, Wohlgemuth, and the American School of Psychologists. In conditions in which clinicians desire to test the nature and the integrity of the reproduction associations, it is not always possible to utilize the methods which psychologists have found valuable; and even when such methods are practicable, there is no standard by which to measure the results they yield. Of the various methods, that which deals with the reproduction associations of stimulus words has been most used by psychiatrists. From the nature of the response and from the time taken to utter it, deductions have been drawn regarding associative complexes in health and disease. But words are uncertain quantities. The result in word associations is seldom inevitable. We instruct and exhort, but we cannot compel investigated persons to utter the first word, to name the first thing, which rises to consciousness after they receive the stimulus word. This first word may, therefore, elude us. Several persons, even the same person at different times, may give different responses to the same word. Davis and Rosanoff have shown that these different responses are sometimes finite in number; that a given stimulus word, in normal people, may excite the reproduction of a member of a small group of words which are its common reactions. But the feeling tone of a word varies in

different persons and even in the same person, according to mood. We cannot, therefore, place any absolute value upon particular reproduction associations of words. We cannot use anomalies of word reproduction as guides to the relative mental states of different persons. We cannot even use such anomalies as an index to variation in the mental state of an individual. Word associations may give clues to associative complexes, they do not afford a measure of the mental peculiarities of persons and are of little diagnostic value. Word associations reveal little regarding the relative importance of perception, of the psychic reinforcement of sensations, and of the affective element of sensations, in the dominance of attention.

Figure associations are fixed by the mordant action of years of use. The memory of a figure, such as 5, is compounded of an impression of a certain shape upon the visual cortex, of a certain sound upon the auditory cortex, of the idea of "fiveness," of a certain motor arrangement in the speech area, and of a certain motor arrangement in the area of the forearm and fingers. The resultant of these visual, psychic, auditory, and kinesthetic images forms our mental picture of five, a picture which from infancy to old age, in the ignorant and in the educated, varies inconsiderably. Special attributes may be associated with certain numbers in the minds of certain people; 7 may be lucky, 13 may be unlucky, 3 may connote the Trinity, and so forth. But the feeling tone of a number is negligible compared with that of a word, and is too significant to be affected by moods. Figure associations are thus more or less fixed and without feeling tone; they have a desirable simplicity; they can be tested in the young, in the old, in the normal, and in the abnormal; for the investigations of mental processes, they have, therefore, in many respects, a greater value than word associations.

When a person sees a printed numeral, for example, 5, and copies it, the seeing and the writing are undeniably related. As the 5 written is identical with the 5 seen, the digit seen is the source of the digit written, and the digit written is the effect of the digit seen.

When a person sees together two different digits, say, 5 and

3, he may write neither, but a third, such as 8. This written digit bears to the pair seen a relation which experience accepts as linking the three digits; thus, 8 is related to 5 and 3 as a sum is to its summands. 8 expresses the result of adding 5 to 3; the digits seen (5 and 3) are again the source of the digit written (8); the digit written (8) is again the effect of the digits seen (5 and 3). To the intellectual processes involved in a simple act of reckoning of this kind, we have, therefore, two guides—the excitant, the digits seen; and its externalized result, the digit written.

If the figure associations are adjusted, tuned, or set for a definite end; if simple addition, subtraction, multiplication, or division be decreed; the digit written is the sum, the difference, the product, or the quotient, respectively, of the digits seen; hence, the same excitant can produce several results. We can deliberately adjust the linking of digits so that the digits seen constantly evoke the effect of one relation. We can so arrange that the only figure relation to yield an externalized result shall be, for example, the relation of addition; in other words, that the figure associations shall be linked so that the digits seen shall excite the writing only of their sums; that the figure associations shall, indeed, be *wholly constrained.* If the associations of the digits seen be wholly constrained, so that the digits written should be, say, sums only, and if, then, a result which is not conformable with this existing intent for addition, a result which is not the sum of the concerned digits, arise, another possible associative reaction of these digits in the presence of this adjustment or set for addition, is revealed. When this aberrant reaction occurs, the digits seen have not been properly guided towards their intended goal; the existing intent or "set" for addition has been temporarily ineffective; what we call attention must, for the moment, have failed in its directive facilitation of the association, since an alien associative reaction or error has arisen.

If, at various times, from the same pair of digits, while the associations are wholly constrained, different errors arise, these several errors represent the externalization, under comparable circumstances, of just so many different possible associative

reactions of this pair of digits. I have endeavored, by studying these irregular associative reactions or errors, to ascertain what happens to the digits which act as visual excitants, and what determines the errors written in reckoning; in other words, I have utilized errors as guides in exploring the mental processes of reckoning.

Our knowledge of errors in adding, we owe to Amberg and Kraepelin and Rivers. They distinguished 'writing errors' —due mainly to increased psychomotor excitability, causing false figures to be unconsciously begun or even completed; and 'thought' errors—due to false associations. Errors were also classified as 'corrected' and 'uncorrected.' Kraepelin and Rivers calculated that of sixty-eight uncorrected and four hundred and three corrected errors they analyzed, 12.5 per cent. were 'thought' errors; 55.4 per cent. were 'writing' errors; and 32.1 per cent. were indeterminable. The most frequent uncorrected error, they found, was the subtraction of a digit instead of its addition; this occurred in 21.32 per cent. of their uncorrected and in 10.92 per cent. of their corrected mistakes. The mere copying of one of the two summands instead of the writing of their sum was met with in 12.5 per cent. of uncorrected, 26.8 per cent. of corrected; no marked tendency to select either the first or the second summands in this error was found. Instead of the proper sum, the sum of the two next digits was found in 10.30 per cent. of the uncorrected errors and in 8.19 per cent. of the corrected. The last sum was added to the following summand in 7.35 per cent. of the uncorrected and in 5.96 per cent. of the corrected. The addition of the newly written sum to the last used sum and sundry other errors, accounted for the remainder which could be interpreted.

I have continued the work they have begun, but have sought to find the significance of the errors; to identify the nature of the errors—anticipations, perseverations, suppressions, etc. My purpose in this was to find not only further information regarding the factors controlling attention, but also to seek an absolute measure of the frequency of associative peculiarities which would be of diagnostic value in abnormal mental states.

Reckoning Test Used.—The reckoning test I used is a modification of that devised by Oehren, and so brilliantly utilized by Kraepelin and his school. It consists of a book, resembling the familiar school copy book, every page of which contains digits, arranged in parallel vertical columns. The space between every two successive digits in a column is equal; between adjacent columns a uniform interval exists; and the margins are of adequate width. Every page has ten columns, each of twenty digits. The changes which I made in the arrangement of the text rendered the ease of writing as constant as possible throughout the page and enabled the number of pairs added to be readily ascertained.

To economize space and gently to complicate the task, the digits are added in pairs *continuously; i. e.*, after a pair of digits has yielded a sum, the second digit of the pair is linked with the succeeding figure in the column, to form a new pair. Each digit (excepting the first in the first column) is thus used twice, for the last digit of one column is paired with the first of the next. Twenty sums are therefore obtained from every column of twenty digits.

Method of Testing.—The test is conducted as follows: The person to be tested is instructed in the method of *continuous* addition. A few seconds' warning is given: then the signal to start. The person adds the digits in pairs as rapidly as he can. At the end of each minute the examining physician calls "Stroke"; the person at once makes a mark under the last sum he has written and proceeds immediately with the addition. By counting the sums between the marks, one can ascertain how many acts of addition, how many units of mental work are done in each minute. The test continues for fifteen minutes.

Key to Test.—I prepared a key printed like the reckoning test, but giving the sums only, in order to facilitate the detection of errors. Every column of twenty sums is printed on a separate slip; the slips are arranged so that the figure columns are vertical and are bound so that the resulting book opens upward not outward; a soft cotton band is interposed between the top of every slip and the binding, so that a slip when

turned up, falls over and lies flat. In number, order, and spacing, the figures on each page of slips tally with the written sums which result from the *continuous* addition of the summands on the corresponding page of the reckoning test. Between each page of slips a blank sheet of paper is inserted to prevent confusion arising from the intermixing of the slips. This modified reckoning test and key were published for me in May, 1911, by Mr. James Mackenzie, Edinburgh, Scotland.

Material Investigated.—I have examined with this modified reckoning test sixty-five persons. Every test lasted fifteen minutes. Fifty-nine persons were examined daily for five to nineteen days each; and the remaining three were examined on two, three, and four days respectively; the total number of tests was 337; the average number of additions accomplished in a test was 751. The total number of acts of addition which I examined was, therefore, about 253,087.

The number of written errors among these 253,087 acts of addition was (4,273 uncorrected, and 7,264 corrected) 11,537.

Many of the written digits were indecipherable, and many were rewritten, especially by persons who improved the figure forms they first wrote. The tendency of some of the investigated was to minimize their alleged mistakes; to interpret the indistinct sums as badly written but accurate results; and to explain corrections as improvements, not alterations. Only decipherable corrected errors were used.

When an act lapsed, an error was certainly made. As most of these lapses occurred at the end of the minutes, when the calling of the word 'stroke' distracted attention from the addition; and, as reveries were sometimes indicated by the lapse of only one act; I did not add the omitted sums to the number of the errors. The errors were, thus, conservatively estimated. The corrected errors exceeded 7,264. 11,537 is, therefore, an index, not to the accuracy with which the 253,087 acts of addition were performed, but only to the number of incontrovertible and decipherable written errors which I examined.

The operation in the test is not merely simple addition. Upon the measure of adding is imposed the novel procedure of

so-called *continuous* addition which I have already explained. The ordinary adjustment for addition must, therefore, be modified to ensure this alteration of the usual manner of adding. As the summands are not isolated in pairs but are printed so as to follow one another down the columns at equal distances, another adjustment is necessary—an adjustment which determines that only the printed summands will be added, and these only in succession and in pairs. To minimize time and effort spent in writing, to make the test an index to the intellectual, not to the muscular processes involved, the one denoting ten in any sum which exceeded 9 was omitted in writing. In example 1, the sums, 11, 13, and 15, were therefore written, 1, 3, and 5, respectively. This *abbreviated ciphering* demands still another mental adaptation.

Ex. I.	*Test* Summands.	Sums in Abbreviated Ciphering.	*Interpretation* Summands, Summands, Sums.
	3		
		7	3 + 4 = 7
	4		
		1	4 + 7 = 11
	7		
		3	7 + 6 = 13
	6		
		5	6 + 9 = 15
	9		

The task, therefore, is somewhat intricate. Its complexity, and the stimulating to speed which I practiced by encouraging competition among the investigated persons, were both useful to my purpose, for both tended to diminish accuracy and to reveal errors which under more simple and familiar conditions would not have been disclosed.

To assist the interpreting of the results obtained with abbreviated ciphering, eight persons were examined for five days each and completed 19,213 acts of addition, writing all numbers in full; thus:

Ex. II.	*Test*	*Interpretation*
	7	
	13	7 + 6 = 13
	6	
	15	6 + 9 = 15
	9	

2,614 reckoning acts were obtained from three persons, who were each examined for two days without the use of either abbreviated ciphering or of the continuous method, thus:

Ex. III.	*Test*	*Interpretation*
	7	
	13	7 + 6 = 13
	6	
	9	
	13	9 + 4 = 13
	4	

Figure Associations in General.—Study of the errors brings sharply before us certain properties of the associations existing between figures to which I may here for a moment refer.

One of the most primitive figure associations is the association of sequence. Digits in sequence are primitive engraved associations. Another fundamental association is that of factors. Factors partake also of the peculiarities which belong to the most powerful of the associated figure groups. Every act of addition is concerned with three chief digits, the two summands and their sum; the three together constitute an *associated figure group*. The number of mutual associations enjoyed by the digits of a group is an index to the associative strength of the group. Thus, the digits 3, 5, and 8 are very specially related. $8 + 5 = (1)3$; $8 - 3 = 5$; $5 \times 3 = (1)5$; $3 \times 5 = (1)5$; $8 - 5 = 3$; $3 + 5 = 8$; $5 + 3 = 8$. Eight possible combinations of two of these digits are thus associated with the third of them. 3, 5, and 8 have therefore an exceptional number of mutual associations; the group 3 5 8 has, therefore, considerable associative strength.

4 5 9 is an analogous group, and has the same associative peculiarities as 3 5 8. ($4 + 5 = 9$; $5 + 4 = 9$; $9 - 4 = 5$; $9 - 5 = 4$; $9 + 5 = (1)4$; $5 + 9 = (1)4$; $5 \times 9 = 45$; $9 \times 5 = 45$.)

A group of three digits, such as 3 5 8, which represents eight completed acts, we may distinguish as an *octad group*. As 4 5 9 represents seven acts, it may be termed a *heptad group*. 7 2 9 is a *quadrad group*. All other groups can thus be conveniently distinguished according to the number of their mutual associations, according to their associative strength.

Errors and Their Mechanism.—We shall first examine the errors which occurred with the digits 3, 5, and 8. Every error which will be examined occurred frequently, but for the sake of simplicity and clearness, I shall cite the minimum number of examples which will suffice to make plain the nature of whatever particular error is under discussion. Errors of a character similar to those with 3 5 8 happened with other associated figure groups; but by examining mainly errors arising from this one group, 3 5 8, we shall make the subject simpler to expound.

Suppression.—If the summands be distinguished as a, b, c, d, the method used in this reckoning test requires that after the act, $a + b = \overline{a + b}$, is completed, b be associated with c to yield $\overline{b + c}$; similarly, c must then be associated with d to yield $\overline{c + d}$; thus:

Ex. IV.

$$\begin{aligned} a \\ &= \overline{a + b} \\ b \\ &= \overline{b + c} \\ c \\ &= \overline{c + d} \\ d \end{aligned}$$

Ex. V.	*Test*		*Interpretation*
	(*a*)[1] 3		
		8	3 + 5 = 8
	(*b*) 5		
		12?	5 + 7 = 12
	(*c*) 8		
		15	8 + 7 = 15
	(*d*) 7		

Here the sum 8 ($a + b$) was correctly suppressed. Although the strength of the tie which binds 5 and 8 is indisputable, yet 5 (*b*) was not added to the summand 8 (*c*)—as the task demanded—but to the summand 8's successor (*d*), the summand 7; the error 12 resulted. The sum 8 ($a + b$) must, therefore, have been actively suppressed. Only thus can we explain the ignoring of the summand 8 (*c*). For the summand 8 thus to be ignored, its form must have been recognized, at

[1] The letters are not part of the printed text. They are inserted here only to facilitate the description.

least to the extent necessary to establish the identity of the printed summand 8 with the written sum 8. *Suppression may affect the digit suppressed so that the seeing of that digit results only in the associations necessary for the detection of its form*, and not in those essential to the customary linking of the digit seen with its figure association. Note that 3 and 5 yielded a motor response 8, although 8 occurred as the next summand (cf. example VIII.).

Ex. VI.	*Test*	*Interpretation*
	(*a*) 3	
	8	$3 + 5 = 8$
	(*b*) 5	
	(*c*) 7	
	11	$7 + 4 = 11$
	(*d*) 4	

Here $\underline{3 + 5} = 8$ was correctly accomplished; then the act $(b + c = \overline{b + c})$ $5 + 7 = 12$, not only failed, but an hiatus occurred in the reckoning; the whole act lapsed. The suppression of 8 $(a + b)$ and 3 (a) suppressed the 5 $(\underline{b})$ also. Any fresh perception of the 5 after the act $a + b = \overline{a + b}$ proved ineffective. At this moment, 7 was apparently completely shut out from the figure associations, for it was not even copied (see examples VII., XX.). *Hence, on the completion of an addition act, all associations of the digits implicated in that act may be suppressed completely.*

Ex. VII.	*Test*	*Interpretation*
	8	
	11	$8 + 3 = 11$
	3	
	7?	$7 = 7$
	5	
	12	$5 + 7 = 12$
	7	

Here 8 and 3 were correctly added, and 3 and 5 yielded no sum. This we know could not be due to lack of associative affinity between 3 and 5. The suppression of $8 + 3 = 11$ must have suppressed 5. Probably the essential cause was the suppression of (*a*) 8, the sum of 3 and 5 (cf. example V.).

Example VII. proves that *the suppression of a digit tends to suppress also the association of those digits habitually associated with it;* that suppression affects the concept of a digit and all its associations; and that, although introspection encourages the supposition that the dissociation of b from the act $a + b = \overline{a + b}$ is attained by the fresh perception of b associated with c, yet any such retroactive inhibitory influence upon $a + b = \overline{a + b}$, must be preceded, at least in certain cases, by an active dissociation of b from the other digits of the just completed act; or by the cessation of the suppression.

Ex. VIII.	*Test*	*Interpretation*
	(*a*) 7	
	12	7 + 5 = 12
	(*b*) 5	
	9?	? = 9
	(*c*) 3	
	11	3 + 8 = 11
	(*d*) 8	

Here 7 and 5 were correctly added. Then 9 was written where 8 should have been. *The motor expression of the sum of 5 and 3 was suppressed before it was written.* The suppression of 8, therefore, was not *the result* of a motor reaction. The premature suppression of the act 5 + 3 = 8 before any motor reproduction was achieved was due to the perception together of the summands 5, 3, and 8 (*b*, *c*, and *d*). *The suppression emanated from the antemotoric figure associations;* it inhibited the reproductive associations of the digits of the completed act; it inhibited the reproduction of 8 as the sum of 5 and 3.

Whence came the 9 written instead of 8 $(\overline{b + c})$? Among the summands, 7 (*a*) and 8 (*d*) occur. 7 8 9 are in sequence. Summand 7 and sum (1)2 might, by addition, yield (7 + 2) 9. There are two tendencies discharged by the writing of 9; an addition (7 + 2) and a sequence (7 8 9) tendency. The suppression of 8 was incomplete, but its figure associations were limited to the reinforced elementary relation of sequence.

Ex. IX.	*Test*	*Interpretation*
	3	
	8	3 + 5 = 8
	5	
	9?	? = 9
	7	
	13	7 + 6 = 13
	6	

Here the act 5 + 7 had no motor expression. Instead, 9 was written. 9 could not have arisen from any addition of these digits except 3 and 6. But 5 6 7 8 9 are in sequence. Suppression permitted the figures to associate in the elementary relation of sequence. 9 conforms to addition, to sequence, and to factors (3 6 9). The writing of 9, therefore, discharged three tendencies.

Ex. X.	*Test*	*Interpretation*
	3	
	8	3 + 5 = 8
	5	
	10?	3 + 7 = 10
	7	
	16	7 + 9 = 16
	9	

Here again 5 and 7 were not added. 10 was written instead of 12. 3 + 7 = 10; but 10 is not merely a sum; it continued the sequence 7 8 9. The act 5 + 7 had no motor expression. There are two tendencies discharged by the writing of 10, an addition (3 + 7), and a sequence tendency (7 8 9 10).

When digits are improperly suppressed, the association in pairs and the set for addition partially fail. A superficial, an external, a primitive figure association, such as sequence, may occur, when the more elaborate or internal figure associations are thus suppressed. Note that when these external associations are reproduced, they generally discharge the affect of more than one associative tendency.

Ex. XI.	*Test*	*Interpretation*
	8	
	11	8 + 3 = 11
	3	
	5?	8 + 7 = (1)5
	1	8 − 3
	8	
	7	1 + 7 = 8

Here the act 8 + 3 = 11 was suppressed. Its suppression affected the summands 3 and 1. 3 was, therefore, not added to 1 to yield 4. 5 resulted instead. The error, 5, emphasizes the tendency of any two (8, 3) of an associated group (3 5 8) to elicit the third (examples IX. and X.); and the tendency of

the motor response under complicated conditions to purge the association sphere of all disturbing associations; the written response is thus the resultant with the greatest affect.

But 11 here induced the suppression of 1. The visual image of 1 must therefore be an integral part of the image of 11. In the visual form associations, 11 must be represented not by a special form, but by a duplication of 1. The suppression of sums, such as 13, 14, 15, 16, or 17, may be observed to suppress the succeeding summands, 3, 4, 5, 6, 7, respectively. Numbers greater than 9, numbers represented by more than one digit, have no individual visual form associations, but are compounded of the visual images of 0 to 9.

Ex. XII.	*Test*	*Interpretation*
	3	
	8	$3 + 5 = 8$
	5	
	16?	$8 + 8 = 16$
	8	
	9	$8 + 1 = 9$
	1	

Here $5 + 8 = 13$ should have occurred. Instead, 16 was written. The sum 8 should not have been added at all; yet it *was* added to the summand 8, for 16 was written instead of 13.

Ex. XIII.	*Test*	*Interpretation*
	(*a*) 3	
	8	$3 + 5 = 8$
	(*b*) 5	
	8?	$8 = 8$
	(*c*) 8	
	4	$8 + 6 = 14$
	(*d*) 6	

Here the suppression of 3, 5, and 8 (a, b, and $\overline{a + b}$) prevented the proper linking of 5 (b) and 8 (c) to yield 13 ($\overline{b + c}$); and caused the summand 8 (c) to be copied.

The 8, in this example, owes its reproduction to its duplication. Under similar circumstances, in example XII., the two 8's were added to yield 16; and, in example V., the second 8, the summand 8, was ignored. 5 and 3 under similar circumstances are subject to analogous effects.

The reproduction of 8 may be deemed an external associa-

tion. Mere duplication of a digit was never observed unaided to cause copying; invariably some objective figure arrangement which was demonstrably capable of eliciting a suppression fault was present at the same time. The evoking of 16 by two 8's is an addition association, and a factor association. Copying, sequence results, and the addition of identical digits are more primitive and more external figure associations than those required, as a rule, by the task.

When 8 was ignored, its associations must have been suppressed to a greater degree than when 8 was copied. When the two 8's were added, their associations must have been more numerous than when 8 was merely copied. Hence, suppression may vary in degree.

Ex. XIV.	*Test*	*Interpretation*
(*a*)	3	
	8	$3 + 5 = 8$
(*b*)	5	
	15?	$8 + 7 = 15$
(*c*)	7	
	11	$7 + 4 = 11$
(*d*)	4	

Here the written sum 8 was not suppressed, but associated with the summand 7 to yield 15. An addition association which contains 5 (*b*) was evoked. The addition of the sum $\overline{a + b}$ to the summand c may occur also when the result does not contain either a, b, or $\overline{a + b}$, (3, 5, or 8).

Numerous examples show that the most difficult digit to suppress of any three, of which two are related to the third as summands to sum, is the sum (examples VIII., IX., X., XII., XIV.). The summands a and b, persist less often perhaps because in the writing of the sum $\overline{a + b}$, the affects of a and b are in part discharged. The persistence of the sum $\overline{a + b}$ is due sometimes, as is seen with the sum 8 of 3 5 8, to the strength of the resistance to suppression engendered by the many associations of the sum and its summands. The persistence of the sum may be due to its concept being elicited by an intellectual image, reinforced by an objective motor image and strengthened by an objective visual image. The examples in which the sum persists to add itself to an identical

succeeding summand or to yield with a succeeding summand, a result which is in sequence, show that the efficacy of the suppression may be influenced by the degree to which the digit to be suppressed habitually associates with the summands which succeed it.

Failure of Suppression: Perseveration

Ex. XV.	*Test*	*Interpretation*
	3	
	8	$3 + 5 = 8$
	5	
	13?	$8 + 5 = 13$
	7	
	8	$7 + 1 = 8$
	1	

Here 13 was written instead of 12. 5 and 7 were not added. The 7 was ignored. 8 was added to 5 to yield 13, a correct addition, but uncalled for by the task. Then 7 and 1 were correctly added. The act of adding 8 and 5 replaced the adding of 5 and 7. The associations of the group 3 5 8 perseverated and achieved a superfluous motor expression.

Ex. XVI.	*Test*	*Interpretation*
	3	
	8	$3 + 5 = 8$
	5	
	11?	$8 + 3 = 11$
	7	
	16	$7 + 9 = 16$
	9	

Here 11 was written in place of 12. The 7 was again ignored and the act $8 + 3 = 11$ replaced $5 + 7 = 12$. $8 + 3 = 11$ is superfluous and is an error not in addition but in the order of adding.

In examples XV. and XVI., addition in pairs persisted. No hiatus (cf. example VI.) occurred in the reckoning. The combination of 5 with 7 was stillborn. These last two examples show that the digits of this group 3 5 8, in contravention of the existing set for addition of the printed summands alone, and of successive summands only, may perseverate, may evoke more than one motor manifestation. The combining of the digits 8 and 5, and 8 and 3 respectively, in the group 3 5 8

was more powerful to excite a motor response than was either the correct association of 5 and 7, or any combination of the digits of the group 3 5 8 with 7. *The 3 5 8 associations in these two examples perseverated.*

As we have seen, and as we shall repeatedly see throughout this paper, the addition of 5 to 7 under certain circumstances is attended by many and diverse errors. To combine 5 with 7 sufficiently to evoke a motor expression common to both is fraught with particular difficulty in these examples. In these examples, 5 is conjoined with the group 3 5 8. 5 not occurring with this group or with an analogous group, such as 4 5 9, combines, as a rule, freely and correctly with 7.

In examples XV. and XVI., suppression failed and the associated group 3 5 8 achieved a superfluous second motor expression. Digits such as 3 5 8, 4 5 9, 3 6 9, etc., which are habitually linked together, tend not only to be suppressed together, but by virtue of their intimate associations tend to resist suppression to an unwonted degree.

In example VII., the suppression of 8 and 3 suppressed also the 5. The more intimately three digits are associated, the more does the suppression of any one tend to suppress the other.

When the reckoner sees two summands, the written result depends not only upon the integrity of the fixed association which links these two summands, not even upon the ease with which these summands associate, but, at least, in part, also upon the force, the accuracy, and perhaps the duration of the suppression of the digits to be discarded from the preceding act of addition. The force and the accuracy of the suppression of the digits to be discarded depends upon the intimacy of the mutual relations between these digits and upon the affinity of the digits suppressed to the succeeding *already seen* summands. *The more numerous the associations of a digit, the more difficult is the suppression of that digit.*

Anticipations and Repetitions

Ex. XVII.	*Test*	*Interpretation*
	2	
	5	2 + 3 = 5
	3	
	2?	5 − 3 ⎫
	8	3 + 9 ⎬ = (1)2
	2?	2 ⎭
	3	
	2	
	9	
	4	9 + 5 = (1)4
	5	

3 and 8 were not added to yield 11. Instead, 2 was written and *thrice repeated;* once in place of 3 + 8 = (1)1, once in place of 8 + 3 = (1)1, and once in place of 3 + 9 = (1)2. The suppression of the act 2 + 3 = 5 must have affected 3 and 8, for neither 3 and 8, nor 8 and 3 yielded a motor response. Apparently, 3 5 8 were associated and 8 was not written as a sum because it already appeared as a summand. Of 2, 3, and 5, 3 and 5 were more affected by the suppression than 2, for 2 was written thrice.

Ex. XVIII.	*Test*	*Interpretation*
	3	
	8	3 + 5 = 8
	5	
	9?	
	7	7 + 2 = 9
	9	
	2	

Here an error has arisen from the difficulty of linking 5 with 7 after the group 3 5 8. No response was yielded by 5 and 7; but instead, 7 was prematurely added to 2 to yield 9; and the sum 9 was repeated. Note 9 is also in sequence (7 8 9).

Ex. XIX.	*Test*	*Interpretation*
	(*a*) 5	
	3	5 + 8 = (1)3
	(*b*) 8	
	2?	5 − 3 =
	(*c*) 3	(1)2
	2	3 + 9 =
	(*d*) 9	

Here the suppression of 5 (*a*), 8 (*b*), and 3 ($\overline{a+b}$), suppressed also 8 (*b*). 8 (*b*) and 3 (*c*) yielded no sum; 11 did not appear. The 3 (*c*) must also have been suppressed, because it was neither copied nor added to any of the preceding digits. The 2 written instead of 11 ($b + c$) may have arisen from $3 + 9$ ($c + d$) = 12, being prematurely added and repeated, or from $5 - 3 = 2$. Anticipation and repetition may both have occurred here. The erroneous repetition of a sum (such as 2 or 9) is not rare in reckoning. I think that when a state of psychomotor irritability exists and no fresh motor picture is stimulated, repetition of the just written figure occurs. The affect of the motor figure picture evoked seems to be incompletely discharged by the writing of a single motor reproduction. Usually an association reinforcing the repeated motor picture (7 8 9 in example XVII. and $5 - 3 = 2$ in example XVIII.) is present.

A similar phenomenon may be seen in the speech of the excited.

When an hiatus occurs, as in example VI., either the psychomotor excitability is less, or the affect of the motor figure picture is less. Perseverations, anticipations, faulty suppressions, and the other errors shown with the group 3 5 8 are equally demonstrable with the groups 4 5 9, 3 6 9, 2 4 8, etc.

The Influence of the Objective Stimuli upon the Attention.—We have already seen that of the digits in the span of apprehension, those specially related tend to be perceived, to be associated together despite the "set" for addition. In the succeeding examples, we shall trace this influence more closely.

Ex. XX.	*Test*	*Interpretation*
	3	
	8	$3 + 5 = 8$
	5	
	7?	$? = 7$
	7	
	9	$7 + 2 = 9$
	2	

3 and 5 were correctly added. The task then required the adding of 5 and 7. Instead of 5 and 7 yielding 12, 7 was written. 7 was written after the digits 3, 5, and 8. No combination of these digits alone or with 7 can yield 7.

First 3 and 5 were added to yield 8, and second, 7 and 2, to yield 9, their respective sums. Two acts of correct addition occurred, one immediately before and one immediately after the error—the writing of 7.

The writing of 7 in error must, therefore, have been due to causes which arose between these two correct acts and must have endured only for the single false act. *The cause of the disturbance must have preceded the error and must have ceased with the error.*

Almost invariably errors are thus isolated. The error as a rule completely discharges the *affect* of the association tendencies which are contrary to the task.

Among ninety-three errors which I examined, which occurred with 8 5 3, 8 3 5, 5 3 8, 3 5 8, and 3 8 5, as successive summands not succeeded by 7; and which were the only decipherable errors associated with the digits 8, 5, and 3 as summands, 7 did not appear. Hence, we may conclude that the writing of 7 in example XX. did not arise from any combination or association of the digits 3, 5, and 8 together.

In the twenty-three examples, all I collected, of errors associated with the successive summands 3, 5, and 7, and 5, 3, and 7, every error was traceable to these digits. No error arose in which an extraneous digit, a digit not derivable from one or more of those concerned, appeared.

As no combination of 3, 5, and 8 will yield 7; as 7 is not derivable from 3, 5, and 8; and as digits extraneous and unrelated to those seen are not written, the 7 written in example XX. must have been derived from the 7 seen; in spite of the existing set to associate digits in pairs and to reproduce their sum, the 7 seen *must have reproduced itself.*

3 5 8 is only one of several groups in connection with which this phenomenon may be observed.

Ex. XXI.	*Test*	*Interpretation*
	4	
	9	4 + 5 = 9
	5	
	7?	7 = 7
	7	
	11	7 + 4 = 11
	4	

Here with the group 4 5 9, 7 is copied as in example XX. This group has the same associative peculiarities as 3 5 8.

Ex. XXII.	*Test*	*Interpretation*
	3	
	9	3 + 6 = 9
	6	
	7?	7 = 7
	7	
	13	7 + 6 = 13
	6	

Here with 3 6 9, 7 is again copied. The group 3 6 9 is also analogous in its associative properties to 3 5 8. Analogous responses to digits other than 7 are made with these and other groups.

The more closely a group of digits is associated, the more isolated must every member of that group be from all other digits. Hence, a digit alien to, and immediately following, an associated group (7 succeeding 3 5 8, example XX.) tends to be isolated and copied, and the motor manifestations of an associated group tend to continue (examples XV. and XVI.). The perseverative errors in examples XV. and XVI. are, therefore, due, at least partly, to the strength of the association which binds 5 to the group 3 5 8. The strength of association conferred by the task upon 5 and 7 in these instances was feebler than that which linked 5 to the group 3 5 8.

Under identical circumstances 7 was reproduced (example XX.), 7 was ignored (examples XV. and XVI.), and the result of the addition of 7 to its succeeding summand was anticipated (example XVIII.).

When 7 was ignored, its difference from 3, 5, and 8 must have been appreciated. Its visual image must have been established, at least to the extent necessary to detect this difference. *The identification of difference in the visual form of digits must, therefore, require weaker associative strength than is necessary to elicit the figure associations essential to motor reproduction.*

As 7 can be passed over during reckoning (examples XV. and XVI.), the objective visual stimulus must excite figure associations, at least, to a certain minimum degree, to procure

either the motor reproduction of a digit or any motor effect whatsoever. The stimulus to reproduction of a digit during reckoning must therefore depend upon the strength of the figure associations.

When 7 was reproduced, its recognition must have been more complete than the mere establishing of its difference from the digits 3, 5, and 8.

When 7 was added to its succeeding summand and achieved a premature result or participated in a sequence, its successful self-assertion must have been due to the strength of its associations with the already seen, simultaneously perceived digits.

From these examples, it is obvious that the figures seen are not limited to those which should be actively concerned in the addition at any given moment. Figures do not strike the retina singly. A certain small number are seen sufficiently to permit recognition of their form and relative position. Within this "span of apprehension," digits between which special associative properties exist tend to react on account of these properties, despite any existing set of contrary associative tendencies. In example VI., 3 and 5 were linked to the exclusion of 7; in example VII., 8, 3, and 5 were linked; in example VIII., 5, 3, and 8 were linked; in example IX., 7 was not linked with 5 but with 8 and 9; in example XX., 7 was isolated and copied; in example XVIII., 7 was linked with the succeeding 2 and achieved a premature result. Hence, we may conclude *that digits which habitually associate together, tend to be perceived together;* that the associative strength of a pair of digits is evident even in the linking of the visual images of these digits. Hence, what I may call scouting sometimes results:

Scouting:

Ex. XXIII.	*Test*	*Interpretation*
	3	
	8	3 + 5 = 8
	5	
	10?	5 + 5 = 10
	7	
	12	5 + 7 = 12
	5	
	14	5 + 9 = 14
	9	

Here no response was yielded by 5 and 7; but instead, 5 was added to 5 to yield 10.

Ex. XXIV. *Test*	*Interpretation*
2	
8	$2 + 6 = 8$
6	
13?	$8 + 5 = 13$
5	
9	$5 + 4 = 9$
4	

Here the sum 8 should have been suppressed and the summands 6 and 5 should have been added to yield 11. Instead, 8 persisted, associated with 5, yielded 13, and completed the group 3 5 8.

Ex. XXV. *Test*	*Interpretation*
2	
5	$2 + 3 = 5$
3	
8?	$5 + 3$
6	$2 + 6$ $\Big\}$ $= 8$
13?	$3 + 5$
5	
	$8 + 5 = 13$

Here the sum 5 should have been suppressed and the summand 3 should have been added to 6 to yield 9. Instead, 5 persisted, combined with 3, yielded 8, and completed the group 3 5 8. A precisely analogous error immediately followed; the sum 8 was added to the summand 5 to yield 13. The group 3 5 8 was here twice completed in contravention to the set for the task.

Examples XXIV. and XXV. show that *when any two of the three* digits, 3, 5, and 8, occur, the third is apt to be written, in spite of the established order of the test. Numerous examples prove the force of mutual attraction exerted by digits related as are these three.

We saw that 8, 5, and 3 have an exceptional number of mutual associations. Hence, the more numerous the associations linking two digits, the more inevitable is their association when both are seen. And the more inevitable their association, the more inevitable is a result not peculiar to either, but common to both.

The behavior of the groups 3 5 8, 4 5 9, 3 6 9, etc., proves that the more numerous the associative acts represented by three digits, *i. e.*, the greater the number of combinations of two of three digits expressed by the third, the greater is the tendency of any two of these digits to elicit the third, and the less their tendency to associate with an alien digit (examples XXIV. and XXV.).

Just as any two digits of a group, such as 3 5 8 or 4 5 9, tend to elicit the third of the group, so does the occurrence of digits in sequence or digits which are factors, tend to elicit a result continuing the sequence or factors. We shall here illustrate only the sequence errors.

Ex. XXVI.	*Test*	*Interpretation*
4		
	11	4 + 7 = 11
7		
	16?	? = 16
8		
	17	8 + 9 = 17
9		

The digits 7 8 9 were already in sequence. In this example the sequence is continued, not completed. Here the 16 might have arisen from 7 + 9. The digit 6 in sequence with 7 8 9 is a component of 16. The erroneous sum represents an addition result wholly, and a sequence result partially.

Ex. XXVII.	*Test*	*Interpretation*
8		
	13	8 + 5 = 13
5		
	14?	8 + 6 = 14
6		
	15?	? = 15
8		

Here the 15 could have arisen from no other mechanism than the continuation of the sequence 13, 14, 15 in the sums. The sequence errors may have their source either in the summands or in the sums.

Ex. XXVIII.	*Test*	*Interpretation*
7		
	9	7 + 2 = 9
2		
	8?	? = 8
5		
	9	5 + 4 = 9
4		

The sum 8 cannot arise from the addition of any possible pair of digits here. It completes the sequence 7 8 9. Note that the factors 2, 4, 8 are now present.

Ex. XXIX. *Test*	*Interpretation*
2	
9	2 + 7 = 9
7	
6?	9 + 7 = (1)6
8	
7	9 + 8 = (1)7
9	

Here the sum 9 was added to the summand 7 to yield (1)6. As the sums ran 6 7 and neither 8 7 nor 7 6, they arose from 9 + 7 and 9 + 8, respectively. Addition obviously persisted, as the sums are not 9 8 7 nor 9 8 9, but 9 6 7; and as the duplicated 9 is not written. Hence, addition, when it can produce a result conformable with the sequence, may prevail. In other words, a result peculiar to addition or to sequence may not occur when a result common to both is possible.

Ex. XXX. *Test*	*Interpretation*
9	
7	9 + 8 = (1)7
8	
8?	7 8 9 = 8
7	
7?	9 + 8 = (1)7
6	

The sum 8 was copied as a result of the sequence. The subsequent sum 7 arose from 9 + 8. Had the sequence alone prevailed, (7 8 9), 9 would have been the result and not 7; had copying, 6 would probably have been copied.

Ex. XXXI. *Test*	*Interpretation*
8	
5	8 + 7 = 15
7	
9?	8 + 1 = 9
1	
6	6 = 6
6	

Here, again, addition in erroneous order and copying were both associated with a continuation of the sequences 5 6 7 8 9.

The errors associated with sequence are additions in erroneous order, false sums, and repetition. The invariable result of these errors is that a sequence is continued (example XXIX.), or completed (example XXVIII.), or digits identical with those already written or printed in sequence are written (examples XXX. and XXXI.).

The sequence may be continued upwards or downwards or a member of the digits in sequence may be repeated out of order. I could not determine in which direction the sequence reaction most readily occurs. The tendency of summands in sequence is to stimulate concepts of digits in sequence, but which of the concepts finds motor expression may be determined by circumstances such as reinforcement by a sum from the submerged addition process.

In example XXIX., the sums run 9 6 7 and not 9 8 7; and in example XXX., 9 8 7 6 yield as sums, 7 8 7, not 7 8 9. Hence, the set for *addition in pairs* obviously persists alongside the tendency to sequence and if a result conformable with sequence and addition can arise, it does arise, in defiance of the order of the sequence.

We have seen that of the digits which assert themselves in the sequence 6 7 8 9, 6 and 7 may represent 16 and 17 and may thus conform both to addition and to sequence. But 16 and 17, the sums of 9 and 7, and of 9 and 8, respectively, do not conform to the sequence; only one component of their motor expression, the 6 of 16, and the 7 of 17, harmonize with the sequence. Only in the *motor* pictures of the two acts is there anything in common between them. In the motor pictures, no question of conflicting figure operations exists; a sequence result can reinforce an addition result, and the motor images of the component digits of numbers greater than ten are there represented separately. Numbers consisting of more than one digit, such as 16, are represented in the motor images by two separate associations, one for 1, the other for 6.

As addition conformable with the test may be evident only when a motor image common to the resulting concepts of the sequence and addition associations is present, we may suppose that the other unexpressed additions occur, that the

addition association is as inevitable as the sequence association. Conversely, when digits in sequence do not prevail over the set for addition, the sequence associations occur, but have a weaker affect than the addition associations.

The central fact which sequence mistakes teach *is that addition associations do not err;* only their motor expression errs; only their externalization is faulty. Their externalization is faulty partly because the motor responses, being few, are shared in common by more than one figure concept. Certain objective stimuli may awaken more than one association, more than one figure concept (examples XVII., XXV., etc.). The intensity with which any motor picture is evoked, obviously depends upon the strength of the affect which evokes it. The same motor picture may receive the affects of more than one figure association group. At the transference of the results of the internal figure associations into their equivalent motor images or in the externalization of that motor equivalent, either the motor image of the intrinsically dominant association, or the motor image which, being common to more than one association, is rendered dominant by the summation of the action of these several associations, prevails.

As addition conformable with the test, addition in erroneous order, and results conforming only to the sequence may all occur from the perception of the same group of digits, either the facilitation of the addition associations must be such that an addition result requires no appreciably greater length of time than is required for the result of the sequence; or the delay in the reproduction of figure concepts may suffice to permit the effect of the addition to act synchronously with the more rapid or more powerful association of sequence.

In reckoning disturbances, due to the objective appearance of digits in sequence, both the power of associating digits in pairs and the adding of digits may persist, alongside the tendency to sequence, and when a common motor result can be attained, it is attained. This suggests why $5 + 6$ equalled (1)1, never 4 or 7; for their tendency to the proper written sum was augmented by the fact that 1 was also the difference between 5 and 6; this subsidiary association may have re-

inforced the dominant association. 4 + 5 usually gave 9, because these digits are particularly related in a group of greater associative power than the sequence tendency (cf. the isolation of 7 in association with this group, example XXI.). 8 and 9 practically always yielded (1)7, probably because of the united effect of the sequence association 7 8 9, and the addition association 9 + 8 = 17.

Writing Errors.—A digit was sometimes falsely begun or falsely completed, or a merely purposeless stroke was written. Hence arose errors, for such false sums, although usually detected, sometimes escaped. The most frequent error of this class was 1 for 6 and 4; occasionally 0 for 9; sometimes 2 for 3, and vice versa. It is noteworthy how often these errors occurred in association with sequence and factors, conditions which I have shown cause a weakening in the set or a suppression error. It was when the rhythm of the associations was thus interrupted that the writing errors usually occurred. Commonly, the error was patently wrong, it did not fit any of the kindred sets,—addition,—sequence, factors, or copying. Hence, usually, it did not satisfy the visual critique and was consequently corrected. Occasionally two corrections of a sum, rarely even three were made and remained decipherable. But often the initial error was illegible under the correction, and sometimes even the correction could not be identified. Some of the seeming writing errors were merely sums badly written and subsequently improved. The writing error was therefore in part due to weakening of the set by the objective figure arrangement, in part to lack of attention, and in part to the psycho-motor excitability. The actual difficulty of writing varies with the different digits; thus, 1 is easier to write than 2; this variation also may be a factor in producing writing errors, under excitement.

We see from copying errors that every digit tends to excite its own reproduction. We see from sequence errors, that every digit must also tend to excite the figures contiguous to it; and that two digits in sequence excite a third in sequence. We can see from factor errors that two digits which are factors excite a third, which *may be* a quotient, a product, or a common multiple of the two, but *is* always a factor.

Irrespective of the order in which digits may be seen and in spite of a subjective facilitation for addition, visual images of digits which habitually associate have, by virtue of the strength of that association, an inherent power to direct the mental processes of reckoning. We have also seen that an active power of suppression of figure associations exists, and that the stronger the figure associations, the more do they resist suppression, and the more do they tend to be suppressed together. When the objective arrangement of digits (sequence, factors, etc.) weakens the set for addition, or the insistent associative tendencies of digits lead digits to resist suppression, the written response, the error, is determined not by accident, but by definite forces. These forces result in errors of the nature of omissions, perseverations, anticipations, etc. Certain errors seem to be peculiar to certain states. Thus, under the influence of alcohol, not only the number of errors increases, but errors of a certain nature seem to predominate. I hope further examination will show that the errors, by their nature and by their relative frequency, may afford us, what we now lack, a measure of the associative power which will be of diagnostic value.

Summary

1. The fundamental response to a perceived isolated digit is the reproduction of that digit.

2. This response, during reckoning, emanates from the figure association sphere, and is dependent upon the functional integrity of the figure associations.

3. Two different digits perceived together tend to produce not an effect peculiar to either, but a resultant common to both.

4. Not only recognition of their forms, but also association of the two digits is essential to the eliciting of a common resultant.

5. The greater the number of associations between two digits, the more inevitable is their association together.

6. The more numerous the associations between three digits, the more inevitable is the externalization of the third when the other two are seen.

7. Objective sequence may induce a response in sequence; objective factorization may induce a factor; objective duplication may induce copying; objectively suggested figure groups may be completed; the grouping of the digits when they are perceived may guide their further association, may dominate attention.

8. Suppression of digits is an active mental process and inhibits the association and the reproduction of the suppressed digits.

9. The suppression of a digit suppresses also the digits habitually associated with it.

10. The primitive attributes of a visual stimulus are not suppressed. Visual form is detected in spite of suppression. Among the figure associations, the most primitive are the most resistant to suppression; copying, sequence, and the adding of identical digits (factors) may survive when all other figure associations are absent.

11. The greater the number of associations of a digit, the more difficult is the suppression of that digit.

12. Digits which by virtue of their intimate associations resist suppression tend to continue their motor manifestations.

13. The motor manifestations of imperfectly suppressed digits depend partly upon the associative opportunities which exist at the moment. It is the result and not the operation which, in such cases, determines the motor response. That digit which most completely discharges the affect of the figure associations is written.

14. The nature and the frequency of the errors may serve as a measure of associative capacity.

Ex. I.

Test	*Interpretation*
3 7	3 + 4 = 7
4 1	4 + 7 = 11
7 3	7 + 6 = 13
6 5	6 + 9 = 15
9	

Ex. II.

Test	*Interpretation*
7 13	7 + 6 = 13
6 15	6 + 9 = 15
9	

Ex. III.

Test	*Interpretation*
7	
13	$7 + 6 = 13$
6	
9	
13	$9 + 4 = 13$
4	

Ex. IV.

Test	*Interpretation*
a	
	$= \overline{a + b}$
b	
	$= \overline{b + c}$
c	
	$= \overline{c + d}$
d	

Ex. V.

Test	*Interpretation*
3	
8	$3 + 5 = 8$
5	
12?	$5 + 7 = 12$
8	
15	$8 + 7 = 15$
7	

Ex. VI.

Test	*Interpretation*
3	
8	$3 + 5 = 8$
5	
7	
11	$7 + 4 = 11$
4	

Ex. VII.

Test	*Interpretation*
8	
11	$8 + 3 = 11$
3	
7?	$7 = 7$
5	
12	$5 + 7 = 12$
7	

Ex. VIII.

Test	*Interpretation*
7	
12	$7 + 5 = 12$
5	
9?	$? = 9$
3	
11	$3 + 8 = 11$
8	

Ex. IX.

Test	*Interpretation*
3	
8	$3 + 5 = 8$
5	
9?	$? = 9$
7	
13	$7 + 6 = 13$
6	

Ex. X.

Test	*Interpretation*
3	
8	$3 + 5 = 8$
5	
10?	$3 + 7 = 10$
7	
16	$7 + 9 = 16$
9	

Ex. XI.

Test	*Interpretation*
8	
11	$8 + 3 = 11$
3	
5?	$8 + 7$
1	$= (1)\ 5$
8	$8 - 3$
7	
	$1 + 7 = 8$

Ex. XII.

Test	*Interpretation*
3	
8	$3 + 5 = 8$
5	
16?	$8 + 8 = 16$
8	
9	$8 + 1 = 9$
1	

Ex. XIII.

Test	*Interpretation*
3	
8	$3 + 5 = 8$
5	
8?	$8 = 8$
8	
4	$8 + 6 = 14$
6	

Ex. XIV.

Test	*Interpretation*
3	
8	$3 + 5 = 8$
5	
15?	$8 + 7 = 15$
7	
11	$7 + 4 = 11$
4	

Ex. XV.

Test	*Interpretation*
3	
8	$3+5=8$
5	
13?	$8+5=13$
7	
8	$7+1=8$
1	

Ex. XVI.

Test	*Interpretation*
3	
8	$3+5=8$
5	
11?	$8+3=11$
7	
16	$7+9=16$
9	

Ex. XVII.

Test	*Interpretation*
2	
5	$2+3=5$
3	
2?	$5-3$
8	
2?	$3+9=2$
3	
2	2
9	
4	$9+5=(1)4$
5	

Ex. XVIII.

Test	*Interpretation*
3	
8	$3+5=8$
5	
9?	
7	
9	$7+2=9$
2	

Ex. XIX.

Test	*Interpretation*
5	
3	$5+8=(1)3$
8	
2?	$5-3=$
3	$(1)2$
2	$3+9=$
9	

Ex. XX.

Test	*Interpretation*
3	
8	$3+5=8$
5	
7?	$?=7$
7	
9	$7+2=9$
2	

Ex. XXI.

Test	*Interpretation*
4	
9	$4+5=9$
5	
7?	$7=7$
7	
11	$7+4=11$
4	

Ex. XXII.

Test	*Interpretation*
3	
9	$3+6=9$
6	
7?	$7=7$
7	
13	$7+6=13$
6	

Ex. XXIII.

Test	*Interpretation*
3	
8	$3+5=8$
5	
10?	$5+5=10$
7	
12	$5+7=12$
5	
14	$5+9=14$
9	

Ex. XXIV.

Test	*Interpretation*
2	
8	$2+6=8$
6	
13?	$8+5=13$
5	
9	$5+4=9$
4	

Ex. XXV.

Test	*Interpretation*
2	
5	$2+3=5$
3	
8?	$5+3$
6	$2+6=8$
13?	$3+5$
5	
	$8+5=13$

Ex. XXVI.

Test	*Interpretation*
4	
11	$4+7=11$
7	
16?	$?=16$
8	
17	$8+9=17$
9	

Ex. XXVII.

Test	*Interpretation*
8	
13	8 + 5 = 13
5	
14?	8 + 6 = 14
6	
15?	? = 15
8	

Ex. XXVIII.

Test	*Interpretation*
7	
9	7 + 2 = 9
2	
8?	? = 8
5	
9	5 + 4 = 9
4	

Ex. XXIX.

Test	*Interpretation*
2	
9	2 + 7 = 9
7	
6?	9 + 7 = (1) 6
8	
7	9 + 8 = (1) 7
9	

Ex. XXX.

Test	*Interpretation*
9	
7	9 + 8 = (1) 7
8	
8?	7 8 9 = 8
7	
7?	9 + 8 = (1) 7
6	

Ex. XXXI.

Test	*Interpretation*
8	
5	8 + 7 = 15
7	
9?	8 + 1 = 9
1	
6	6 = 6
6	

CHART OF THE ERRORS DISCUSSED IN THE TEXT.

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